



EXPERIMENTAL INQUIRIES

INTO THE

PROPERTIES of the BLOOD.



Lately Published,
**EXPERIMENTAL ENQUIRIES:
PART THE SECOND.**

Containing a Description of the
LYMPHATIC SYSTEM,
IN THE
HUMAN SUBJECT,

AND IN OTHER
A N I M A L S.

Illustrated with **PLATES.**

By **W. HEWSON, F. R. S.**

[Price 5s. in Boards.]

Experimental Inquiries :
PART THE FIRST.
CONTAINING AN
I N Q U I R Y
INTO THE
PROPERTIES OF THE BLOOD.
WITH
REMARKS on some of its MORBID
APPEARANCES:
AND
AN APPENDIX,
RELATING TO
The Discovery of the Lymphatic System in
Birds, Fish, and the Animals called
Amphibious.
The THIRD EDITION.

By WILLIAM HEWSON, F.R.S.
AND TEACHER OF ANATOMY.

Vere scire, est per causas scire. Lord BACON.

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Experimental Inquiry

into the First

Principle of

INQUIRY

PROPERTIES OF THE BLOOD

RESIDES IN THE MORBID

STATE

AS A FURTHER

INQUIRY

THE THEORY OF THE BLOOD

AND ITS

THEORY OF THE BLOOD

BY WILLIAM B. BROWN

AND

THEORY OF THE BLOOD

AND

THEORY OF THE BLOOD

AND

T O

Sir JOHN PRINGLE, Bart.
PHYSICIAN TO HER MAJESTY.

S I R,

I HAVE taken the liberty to address you upon this occasion, not so much that I might pay a compliment to your merit, as that I might boast of your friendship: It is impossible that I should add any thing to your reputation by displaying your abilities; but you may add greatly to mine, even by detecting my mistakes. You have done me the honour to approve this little Essay, and I am ambitious to tell the world, that it owes

vi DEDICATION.

much both to your knowledge and your kindness ; which have concurred to suggest many useful hints and judicious corrections, that make it more worthy of attention, and afford me a public opportunity of assuring you that I am, with the truest respect and esteem,

S I R,

Your most obedient,

and most obliged

humble servant,

WILLIAM HEWSON.

P R E F A C E.

THE knowledge of the human frame, the preservation of health, and the cure of diseases, are objects of too great importance to mankind, for the Author of these sheets to doubt, that any attempts to promote them, how small soever, should not meet with a candid and indulgent reception from the Public. An Inquiry into the Properties of the Blood, it is presumed, will be thought, in a particular manner, interesting, since there is no part of the human body upon which more physiological reasoning is founded, nor any from which more inferences are drawn for the cure of diseases: And, as the inquiry is made by experiments upon the blood as near as possible to the state in which it circulates in the vessels, it is hoped that the conclu-

sions made from them will stand the test of a candid examination, and lead to further observations and improvements.

SINCE the publication of the first Edition, some new experiments have been made, and a new chapter has been added, which contains a recapitulation of the principal facts and conclusions that are met with in this Essay. These additions are between p. 98 and 140.

THE Appendix is a vindication of the Author's right to the discovery of the Lymphatic Vessels, in opposition to the claim of the learned Dr. Alexander Monro, Professor of Anatomy in the University of Edinburgh.

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A N

EXPERIMENTAL INQUIRY

INTO THE

PROPERTIES of the BLOOD.

C H A P. I.

*Of the separation of the Serum; the colour
of the Crassamentum; and of the causes
of the COAGULATION OF THE BLOOD.*

WHEN fresh blood is received into a basin, and suffered to rest, in a few minutes it jellies, or coagulates, and soon after separates into two parts, distinguished by the names of *Crassamentum* and *Serum*. These two parts differ

B in

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in their proportions in different constitutions; in a strong person, the *crassamentum* is in greater proportion to the *serum* than in a weak one; and the same difference is found to take place in diseases; thence is deduced the general conclusion, that the less the quantity of *serum* is in proportion to the *crassamentum*, bleeding, diluting liquors, and a low diet, are the more necessary; whilst in some dropsies, and other diseases where the *serum* is in a great, and the *crassamentum* in a small proportion, bleeding and diluting would be highly improper. As it is therefore supposed useful to attend to the proportions of these parts in many disorders, and even to take indications of cure from them, it has been an object with those who have made experiments on the blood, to determine the circumstances on which its more perfect separation into these two parts depends; it being obvious, that till this be done, our inferences from their proportions will be liable to considerable fallacies. Two of the

the latest writers on this subject agree, that if the blood, after being taken from a vein, be set in a cold place, it will not easily separate, and that a moderate warmth is necessary: this is a fact that is evinced by daily experience. They likewise say, that the heat should be less than that of the animal, or than 98° of Fahrenheit's thermometer; and that, if fresh blood be received into a cup, and that cup put into water heated to 98° , it will not separate; nay, they even say, that it will not coagulate; but this, I am persuaded from experiments, is ill founded.

EXPERIMENT I.

A TIN-VESSEL, containing water, was placed upon a lamp, which kept the water in a heat that varied between 100 and 105 degrees. In this water was placed a phial, containing blood that instant taken from the arm of a person

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in health; the phial was previously warmed, then filled, and corked to exclude air. In the same water was placed a tea-cup half full of blood, just taken from the same person; a third portion of the blood was then received from the same vein into a basin, and was set upon a table, the heat of the atmosphere being at 67° . Now, according to their opinion, the two former should neither have coagulated nor separated, when that in the basin began to separate; but, on the contrary, they were all three found to coagulate nearly in the same time; and those in the warm water not only did separate as well as the other, but even sooner.

EXPERIMENT II.

THE same experiment was repeated on the blood of a person that laboured under the acute rheumatism, whilst the heat of the atmosphere was no higher than 55° , and that of the warm water was 108° ;
and

and the result of this experiment was not only a confirmation of what was observed in the first, but it even shewed, that this degree of heat was so far from lessening, that it increased the disposition to coagulate; for the blood in the cup and in the phial was not only congealed, but the separation was much advanced before the whole of the blood in the basin was coagulated. Thence I am led to conclude, that the separation of the blood in a given time, is in proportion as the heat in which it stands is nearer to the animal heat, or 98° ; or greater in that heat than in any of a less degree. And I am confirmed in this inference by experiments hereafter to be related, where the blood in the living animal, whilst at rest, was found both to coagulate and to separate.

It is well known, that the *crassamentum* consists of two parts, of which one gives it solidity, and is by some called the fibrous part of the blood, or the *gluten*,

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but by others with more propriety termed the *coagulable lymph*; and of another, which gives the red colour to the blood, and is called the *red globules*. These two parts can be separated by washing the *crassamentum* in water, the red particles dissolving in the water, whilst the coagulable lymph remains solid. That it is the coagulable lymph, which, by its becoming solid, gives firmness to the *crassamentum*, is proved by agitating fresh blood with a stick, so as to collect this substance on the stick, in which case the rest of the blood remains fluid*.

* It may be proper to mention here, that till of late the coagulable lymph has been confounded with the serum of the blood, which contains a substance that is likewise coagulable. But in these sheets, by the *lymph*, is always meant that part of the blood which jellies, or becomes solid spontaneously when blood is received into a basin, which the coagulable matter that is dissolved in the serum does not; but agrees more with the white of an egg, in remaining fluid when exposed to the air, and coagulating when exposed to heat, or when mixed with ardent spirits, or some other chemical substances.

THE

THE surface of the *crassamentum*, when not covered with a size, is in general of a more florid red than the blood was when first taken from the vein, whilst its bottom is of a dark colour, or blackish. This floridness of the surface is justly attributed by some of the more accurate observers to the air, with which it is in contact; for, if the *crassamentum* be inverted, the colours are changed, at least that which is now become the upper surface assumes a more florid redness. This difference of colour, others have endeavoured to explain from the different proportions of the red particles, or globules as they are called, which, say they, being in a greater proportion at the bottom of the *crassamentum*, make it appear black; but, if inverted, the globules then settle from the surface which is now uppermost, and that becomes redder. But this I think is not probable; for the lymph in the *crassamentum* is so firmly coagulated, as to make it too dense to allow of bodies even heavier than the

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red particles to gravitate through it ; for
example gold. That air has the power
of changing the colour of the blood, has
been long known ; and the following
experiment shews it very satisfactorily,
and hardly leaves room to refer the ap-
pearance to another cause.

EXPERIMENT III.

HAVING laid bare the jugular vein of
a living rabbit, I tied it up in three
places ; then opening it between two of
the ligatures, I let out the blood, and
filled this part of the vein with air.
After letting it rest a little, till the air
should become warm, I took off the liga-
ture which separated the air from the
blood, and then gently mixed them, and
I observed that the venous blood assumed
a more florid redness, where it was in
contact with the air-bubbles, whilst in
other parts it remained of its natural
colour.

THERE

THERE is a difference between the arterial and venous blood in colour; the former is of a florid red like the surface of the *crassamentum*, the latter is dark or blackish like the bottom of the *crassamentum*. This change in its colour is produced on the blood as it passes through the lungs, as we see by opening of living animals *; and as a similar change is produced by air applied to blood out of the body, it is presumed that the air in the lungs is the immediate cause of this change; but how it effects it, is not yet determined.

* That this change is really produced in the lungs, I am persuaded from experiments, in which I have distinctly seen the blood of a more florid red in the left auricle, than it was in the right. But some authors of the greatest authority say, that they could not observe any such difference in a great number of experiments which they made; but this I should attribute to their having been later in opening the left auricle after the collapsing of the lungs than I was; for it seems probable, that whatever is the alteration produced on the blood in its circulation through this organ, that change cannot take place after it is collapsed.

As

As the blood is changed to a more florid red in passing through the lungs, or from the venous to the arterial system, so it loses that colour again in passing from the arteries to the veins in the extreme parts, especially when the person is in health ; but every now and then we observe the blood in the veins more florid than is usual, and it likewise frequently happens in venesection, that the blood which comes first out is blackish, and that which comes afterwards is more florid : in such cases the arterial blood passes into the veins without undergoing that change which is natural to it.

SOME of the neutral salts have a similar effect on the colour of the blood to what air has, particularly nitre ; thence some have attributed the difference of colour in the arterial and venous blood to nitre, which they supposed was absorbed from the air whilst in the lungs. But we know that this is a mere hypothesis, for air contains no nitre. Indeed
nitre

nitre is far from being the only neutral salt which has this effect on the blood, for most of them have some degree of it. In making some experiments on this subject, I have observed a more remarkable effect which some of the neutral salts have upon the blood; and that is, being mixed with it when just received from the vein, they prevent its coagulation, or keep it fluid, and yet, upon adding water to the mixture, it then jellies or coagulates. Thus, if six ounces of human blood be received from a vein upon half an ounce of true Glauber's salt reduced to a powder, and the mixture agitated so as to make the salt be dissolved, that blood will not coagulate on being exposed to the air, as it would have done without the salt; but if to this mixture about twice its quantity of water be added, in a short time the whole will be jellied or coagulated, and on shaking the jelly, the *coagulum* will be broken, and the part so coagulated
can

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can be now separated as it falls to the bottom, and proves to be the lymph.

IN these mixtures of the blood with neutral salts, the red particles readily subside, (especially if human blood be used) and the surface of the mixture becomes clear and colourless; and being poured off from the red part, it is found to contain the coagulable lymph, which can be separated by the addition of water.

I HAVE tried most of the neutral salts, and have made a table of their effects on the blood; but it is not necessary to trouble the reader with it, as we do not see of what use it could be in medicine; for we must not conclude that their effects within the body would be the same as out of it*. Indeed, these experiments,

* The salts which keep the blood fluid by itself, and yet allow it afterwards to jelly on being mixed with water, are, *Sal Glauberi verus*; *Sal Digestivus Sylvii*,

periments, as well as some others, were not made so much with a view of any immediate application to medicine, as to determine the properties of the blood chemically; for, having set out with a persuasion, that a more particular acquaintance with the properties of this fluid was necessary before we could arrive at the knowledge of some of the animal functions, such as the manner in which the bile and other secreted fluids are formed, I therefore was anxious to throw some more light on this subject. With this view I have made some experiments even on living animals, being convinced

Sylvii; *Sal Communis*; *Nitrum Commune*; ———
Nitrum Cubicum; *Sal Diureticus*; *Borax*. The salt made of vinegar and the fossile Alkali; and the salt made with Vinegar and Chalk.

The following salts likewise keep the blood fluid, but do not allow it to jelly when mixed with water; *Tartarus Vitriolatus*; *Sal Epsomensis*; *Sal Ammoniacus Communis*; *Sal Ammoniacus Nitrosus*; *Sal Rupillensis*.

But Alum on being mixed with blood coagulates it immediately. that

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that my inquiries would not otherwise be
satisfactory.

WHEN blood is thus kept fluid by Glauber's salt, it still retains its property of being coagulable by heat, and by other substances as before, air excepted. This method of keeping the blood fluid may therefore be useful, by affording an opportunity of making some experiments upon it, which we could not otherwise do from its coagulating so soon when taken from the vessels.

THIS property of one of the neutral salts has been long known amongst those who prepare the blood of cattle for food; for it has long been a practice with such people, to receive it into a vessel containing some common salt, and to agitate it as fast as it falls, by which means the coagulation is prevented, and the blood remains so fluid as to pass through a strainer, without leaving any *coagulum* behind:

behind: by this means they have an opportunity of mixing it with other substances for culinary purposes

ALTHOUGH the coagulable lymph so readily becomes solid when exposed to the air, yet whilst circulating it is far from that consistence: it has indeed been supposed to be fibrous, even whilst moving in the blood-vessels, but erroneously.

It is this coagulable lymph which forms the inflammatory crust, or *buff*, as it is called. It likewise forms *polypi* of the heart, and sometimes fills up the cavities of aneurisms, and plugs up the extremities of divided arteries. It is supposed, by its becoming solid in the body, to occasion obstructions and inflammations; and even mortifications, from the exposition to cold, have been attributed to its coagulation. In a word, this lymph is supposed to have so great a share in the cause of several diseases, that

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that it would be a desirable matter to be able to ascertain the causes of that coagulation, either in the body, or out of it.

THE blood, when received into a bason, and suffered to rest in the common heat of the atmosphere, very soon jellies or coagulates; the part which now becomes solid is the coagulable lymph, as has been shewn above. The circumstances in which it now differs from what it was in the veins, are these: it is exposed to the air, to cold, and is at rest; for whilst in the body, air is excluded, it is there of a considerable warmth, and is always in motion. The question is, to which of these circumstances its coagulation whilst in the bason is chiefly owing. This question, I believe, cannot well be answered from the experiments that have hitherto been made. It has indeed been said, that the cold alone coagulated it; for, say they, if you receive blood into a bason, and set
that

that bafon in warm water, and ftir the blood well, it can be kept fluid. But in the experiments from which this conclufion was made, I find there has been a deception *. In fhort, I have found that it coagulates as foon when kept warm and when agitated, as it does when fuffered to reft and to cool. As the fubject feemed to me of importance, I have endeavoured to afcertain the circumftance to which this coagulation is owing by feveral experiments, in each of which the blood was generally expofed to but one of the fufpected caufes at a time. Thus, in order to fee whether the blood's coagulation out of the body was owing to its being at reft, I made the following experiment :

* That is, the lymph really had been coagulated, but by the agitation had likewise been feparated from the reft of the blood, and had thereby efaped notice.

C

E X P E-

E X P E R I M E N T I V.

HAVING laid bare the jugular vein of a living dog, I made a ligature upon it in two places, so that the blood was at rest between the ligatures ; then covering the vein with the skin, to prevent its cooling, I left it in this situation. From several experiments made in this way, I found in general, that after being at rest for ten minutes, the blood continued fluid ; nay, that after being at rest for three hours and a quarter, above two-thirds of it were still fluid, though it coagulated afterwards. Now the blood, when taken from a vein of the same animal, was completely jellied in about seven minutes. The coagulation therefore of the blood in the bason, and of that which is merely at rest, are so different, that rest alone cannot be supposed to be the cause of the coagulation out of the body.

To

To see the effects of cold on the blood,
I made this experiment :

EXPERIMENT V.

I KILLED a rabbit, and immediately cut out one of its jugular veins, proper ligatures being previously made upon it ; I then threw the vein into a solution of sal ammoniac and snow, in which the mercury stood at the 14th degree of Fahrenheit's thermometer. As soon as the blood was frozen and converted into ice, I took the vein out again, and put it into lukewarm water till it thawed and became soft ; I then opened the vein, received the blood into a tea-cup, and observed that it was perfectly fluid, and in a few minutes it jellied or coagulated, as blood usually does. Now, as in this experiment the blood was frozen and thawed again without being coagulated, it is evident that the coagulation of the blood out of the body is not solely owing to cold, any more than it is to rest.

NEXT, to see the effects of air upon the blood, I tried as follows :

E X P E R I M E N T VI.

HAVING laid bare the jugular vein of a living rabbit, I tied it up in three places, and then opened it between two of the ligatures, and emptied that part of its blood. I next blew warm air into the empty vein, and put another ligature upon it, and letting it rest till I thought the air had acquired the same degree of heat as the blood, I then removed the intermediate ligature, and mixed the air with the blood. The air immediately made the blood florid, where it was in contact with it, as could be seen through the coats of the vein. In a quarter of an hour I opened the vein, and found the blood entirely coagulated ; and as the blood could not in this time have been completely congealed by rest alone, the air was probably the cause of its coagulation.

FROM

FROM comparing these experiments, may we not venture to conclude, that the air is a strong coagulant of the blood, and that to this its coagulation when taken from the veins is chiefly owing, and not to cold, nor to rest?

BUT although it appears from these experiments, that the coagulation of the blood in the basin is owing to the air alone; for cold has no such effect, nor has rest in a sufficient degree, because the coagulation of the blood in the basin takes place in a few minutes, whilst that which is merely at rest in the veins is not completely coagulated in three hours or more. Yet the blood is in time completely coagulated merely by its being at rest in the veins; but then in this case it coagulates in a different manner from what it does in the basin; and as it probably is in this way that the blood is coagulated in the body, I have been more particularly attentive to it, and have endeavoured to determine by

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experiment how it takes place. With this view I have several times repeated Experiment the 4th, which was made with a view to determine whether the blood would coagulate by rest. In the first trial, the vein was not opened till the end of three hours and a quarter; and just before it was opened I had observed through its coats, that the upper part of the blood was transparent, owing to the separation of the lymph. On letting out this blood, it seemed to me entirely fluid; a part indeed had been lost, but the greatest part was collected in the cup, and which afterwards coagulated as blood commonly does when exposed to the air. From this experiment I imagined that the whole had been fluid; but from others made since, I am persuaded that the part which was lost had been coagulated; for, from a variety of trials, I now find, that though the whole of the blood is not congealed in this time by rest alone, yet a part of it is. But as it would be trespassing too much

much on the reader's time to relate every experiment I have been obliged to make for this purpose, I shall only mention the general result of the whole.

AFTER fixing a dog down to a table, and tying up his jugular veins, I have in general found, that on opening them at the end of ten minutes, the blood was still entirely fluid, or without any appearance of coagulation *. If they were opened at the end of fifteen minutes, at first sight it also appeared quite fluid; but on a careful examination I found sometimes one, and sometimes two or three small particles about the size of a pin's head, which were coagulated parts of the blood. When opened later than this period, a larger and larger

* I say *in general* it was fluid at the end of ten minutes; but I must likewise mention that in one dog I found two very small particles of beginning coagulation, even at this period; yet in another I could not observe any such appearance, even at the end of fifteen minutes.

coagulum was observed ; but so very slowly does this coagulation proceed, that in an experiment where I had the curiosity to compare more exactly the clotted part with the unclotted, I found, after the vein had been tied two hours and a quarter, that the *coagulum* weighed only two grains ; whilst the rest of the blood, which was fluid, on being suffered to congeal, weighed eleven grains. I can advance nothing farther in this part of my subject with precision. Nor can I pretend exactly to determine the time at which all the blood between the ligatures is coagulated. I have indeed opened such a vein at the end of three days, when I found a thin, white *coagulum*, which was a mere film ; the *serum* and red particles having disappeared. But the whole is undoubtedly congealed long before this period. The manner in which the blood coagulates, when at rest in the body, has appeared to me curious, and therefore I have taken the more pains to discover how it happens, especially

cially as it may assist us in judging whether or no it coagulates in the heart, so as to form those substances called *polypi*. The abovementioned times will, I believe, be found to be those at which the blood congeals in the veins of healthy dogs: and as I have found, by experiments, that the blood of a dog and of the human subject in health jellies out of the body, nearly in the same time, that is, it begins in three or four minutes, and is completed in seven or eight; I should therefore conclude that the blood in the veins of the human body coagulates nearly at the same period with that of a dog. But it may be necessary to add here, that from experiments which I have made, I have reason to believe that the time at which the blood coagulates, is different in different constitutions, and in different diseases. For though the blood of a person in health is completely coagulated in seven minutes after it is taken out of the veins, yet in some diseases, I have found the blood fifteen or
twenty

twenty minutes, nay even an hour and an half, before it was completely jellied.

As we see in the above-related experiments, that the blood coagulates in the body when suffered to rest for a little time, is it not probable that to this cause its coagulation in those true aneurysms, which are attended with a pouch, are owing*? For in such enlargements a part of the blood is without motion, which will congeal when at rest, and in contact with the sack; and thus one layer may be formed; and the sack afterwards enlarging, another portion of the blood will then be at rest; and so a second layer may be formed; and thence probably is the origin of those laminated *coagula* met with in such sacks.

LIKEWISE, to the blood's being at rest, is probably owing its coagulation in the

* An instance of which may be seen in the Med. Obs. and Inq. vol. i. article xxvii. fig. iii.

large arteries which are tied after amputation, or other operations; for after most of such ligatures there will be a part of the artery impervious, in which the blood can have no motion. The *coagulum* after amputation might indeed be supposed owing to air; but, considering the manner in which arteries are tied whilst the blood is running from them, it does not seem probable that the air has any effect on what is above the ligature.

To the blood's being without motion in the cavity of the *uterus*, is its coagulation therein probably owing; hence the origin of those large clots which sometimes come from that cavity; and which, when more condensed by the oozing out of the *serum*, and of the red globules, assume a flesh-like appearance, and have often been called *Moles* or *false conceptions*.

IN

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IN Experiment the 5th, we found that the blood could be frozen and thawed again, without being coagulated: this likewise is an experiment which I have repeated several times, that I might be sure of the fact. I have also varied the experiment by sometimes putting the vein into a phial of water, and freezing the whole in a solution of sal ammoniac in snow; and sometimes I have put the vein into the solution itself; and three or four times I have thrown it into oil, and then frozen it; but after all these trials the result was the same. The blood was always evidently fluid on being thawed, and as evidently jellied when exposed to the air.

BESIDES being coagulated when exposed to the air, the coagulable lymph, as well as the *serum*, is known to be fixed by heat; but the degree of heat has not, so far as I know, been determined. It has been supposed to require a degree almost

almost equal to that which coagulates the serum * but one much less is necessary, as will appear from the following experiments.

EXPERIMENT VII.

HAVING found, from a number of trials, that blood, kept fluid by means of true Glauber's salt, had its lymph coagulated by a heat of 125° of Fahrenheit's thermometer, I supposed that the degree necessary for fixing it in its natural state could not be very different from this. I therefore prepared a lamp-furnace with a small vessel of water upon it; this water was heated to 125° ; and then laying bare the jugular vein of a living dog, I tied it properly, cut a piece of it out, and put it into this water: after eleven minutes, I took out the

* See *Traité du Cœur*. T. ii. p. 93, Schwenke *Hæmatolog.* p. 138.

vein,

vein, opened it, and found the blood entirely coagulated; thence I concluded, that 125° , or leis, was sufficient to coagulate the blood of a dog. It may be necessary to observe here, that the part coagulated was only the lymph; for the *serum* requires a much greater heat to fix it, that is a heat of 160° , as will appear hereafter.

EXPERIMENT VIII.

THE same experiment was repeated in such a manner, that the heat never went higher than 120° and an half; and I found, that on opening the vein at the end of eleven minutes, the lymph was entirely coagulated, even in this heat.

EXPERIMENT IX.

I REPEATED the experiment with a heat no higher than 114° , and at the end of eleven minutes, the vein being opened,
the

the blood was found to be fluid, and in a few minutes after, being laid open to the air, it coagulated as usual. Now, as the blood in the last experiment was coagulated, when the heat had never risen above 120° and an half; and in this experiment was fluid, though it had been exposed to a heat of 114° ; we may therefore conclude, that the coagulable lymph in the blood of a dog, in health, is fixed in a degree of heat between 114° and $120^{\circ}\frac{1}{2}$ of Fahrenheit's thermometer.

As to the degree of heat at which the lymph in human blood coagulates, I have not yet had an opportunity of trying it in a more satisfactory way, than with the mixture with Glauber's salt, in which state it coagulates at 125° . But, as we find that the human blood and that of a dog jelly nearly in the same time, when exposed to the air, I think it probable that the precise degree of heat at which the lymph of the human blood coagulates,

lates, is between 114° and $120^{\circ}\frac{1}{2}$. I have thought of making the experiment on the umbilical cord of a recent *placenta*, which perhaps is the most likely way of coming at the truth.

THE degree of heat, at which the *serum* of the blood (which should not be confounded with the lymph) coagulates, is generally said to be 150° ; but from my experiments I am inclined to believe it requires a greater heat to fix it. They were made in the following manner :

EXPERIMENT X.

I TOOK a wide-mouthed phial, containing some *serum*, and placing a thermometer in it, I put it into water which was kept warm by a lamp underneath; and, in making this experiment with as much accuracy as I could, I found the heat required was 160° ; which is about
forty

forty degrees more than is necessary for the coagulation of the lymph.

As the blood is coagulable by heat, and as the heat of an animal is increased in fevers, it has been supposed that the blood might be coagulated by the animal heat, even whilst it is circulating in the vessels; but there is little foundation for such an opinion, since the animal heat is naturally only 98° or 100° , and in the most ardent fever is not raised above 112° .



C H A P. II.

Of the inflammatory Crust, or Size.

I SHALL next proceed to inquire into the formation of the inflammatory crust, or *size*, as it is called.

THIS remarkable appearance is frequently met with in inflammatory disorders, and is formed by the coagulable lymph's being fixed, or coagulated, after the red particles have subsided. It has indeed been supposed to be formed from the *serum* of the blood; and an excellent writer on this subject seems in doubt to which of the two it should be attributed. But that it is formed by the coagulable lymph alone, after the red particles have subsided, appears from the following experiments.

E X P E-

EXPERIMENT XI.

In the month of June, when the thermometer in the shade stood at 67° , I bled a man who had laboured under a *phthisis pulmonalis* for some months, and at that time complained of a pain in his side. The blood, though it came out in a small stream, yet flowed with such velocity, that it soon filled the basin. After tying up his arm I attended to the blood, and observed that the surface became transparent, and that the transparency gradually went deeper and deeper, the blood being still fluid. I likewise observed that the coagulation first began on the surface, where it was in contact with the air, and formed a thin pellicle; this I removed, and saw that it was soon succeeded by a second. I then took up a part of the clear liquor with a wet teaspoon, and put it into a phial with an equal quantity of water; a second portion

D 2

I kept

I kept in the tea-spoon; and I found afterwards that they both jellied or coagulated, as did also the surface of the *crassamentum*, making a thick crust. On pressing with my finger that portion which was in the tea-spoon, I found it contained a little *serum*.

FROM this experiment it is evident, that the substance which formed the size was fluid after it was taken from the vein, and coagulated when exposed to the air; and as this is a property of the coagulable lymph alone, and not of the *serum*, there can be no doubt that the size was formed of the lymph.

THE following experiment, made on the blood, without exposing it to the air, likewise proves the same fact.

EXPE-

EXPERIMENT XII.

IMMEDIATELY after killing a dog, I tied up his jugular veins near the *sternum*, and hung his head over the edge of the table, so that the parts of the veins where the ligatures were might be higher than his head. I looked at the veins from time to time, and observed that they became transparent at their upper part, the red particles subsiding. I then made a ligature upon one vein, so as to divide the transparent from the red portion of the blood; and opening the vein, I let out the transparent portion, which was still fluid, but coagulated soon after. On pressing this *coagulum*, I found it contain a little *serum*. The other vein I did not open till after the blood was congealed, and then I found the upperpart of the

D 3

coagulum

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coagulum whitish like the crust in pleuritic blood *.

AND that the size is merely the coagulable lymph separated by the subsidence of the red particles, will appear evident to any person who will, as Sydenham directs, move a finger, or a tea-spoon through the blood when he observes its surface becoming transparent; for in this case the blood that otherwise would have been fizy, will now have a natural appearance, or be without size; from the red particles being prevented from subsiding.

* This is not the only apparently healthful animal whose blood had a crust; I have seen it in others: whence I at first suspected that merely keeping the blood fluid for a little time was sufficient to produce this appearance; but I altered my opinion, on seeing, that in the greatest number of animals it did not occur: nor is it commonly met with in the hearts of those persons who die a violent death, though the blood remains longer fluid in such cases, than it does in the basin where the size appears.

IT

It has been a very generally received opinion, that inflammation thickens the blood, and makes it more ready to coagulate. Nay, some have gone so far as to say, that in those disorders where the inflammatory crust is seen, the blood is almost coagulated even before it is let out of the vein. Now I am persuaded from experiment, that the contrary of this is true; or that inflammation, instead of increasing the disposition of the blood to coagulate, really lessens it; and instead of thickening the blood, really thins it; at least that part which forms the crust, viz. the coagulable lymph.

IN the first place, that inflammation really lessens the disposition to coagulate, will appear evident to every one who attends to the jellying of such blood as has a crust. For in all those cases the blood will be found to be longer in congealing, than in its natural state. To this opinion I was first led by attending to the phthical patient's blood abovementioned; but

I have since made a comparison, which seems to prove the fact. For, from a variety of experiments made on the blood of persons nearly in health, or at least who had no inflammatory disorder, and no crust on their blood, I found that after being taken from a vein, it began to jelly in about three minutes and an half. The first appearance of coagulation was a thin film on the surface near the air-bubbles, or near the edge of the basin; this film spread over the surface, and thickened gradually till the whole was jellied, which was in about seven minutes after the opening of the vein; and in about ten or eleven the whole was so firm, that, on cutting the cake, the gasles were immediately filled up by the *serum*, which now began to separate from the *crassamentum*. But in those persons whose blood had an inflammatory crust, the coagulation was much later; as will appear from the following experiments.

E X P E.

EXPERIMENT XIII.

I BLED a woman who was seven months gone with child, and the blood was received into a bason. In five minutes after the vein was opened, a film first appeared; but this spread so slowly, that in ten minutes it did not cover the whole surface; in fifteen minutes it had nearly spread over the surface; but the rest of the blood was quite fluid, at least for some depth, and even in half an hour it was not so firmly jellied as it was afterwards. In this case there was a very thick and strong crust or size.

EXPERIMENT XIV.

HAVING bled a person with a violent rheumatic pain in his breast, the blood was received into three tea-cups, and
each

each of them had afterwards a crust. In the first I observed the progress of the coagulation, as follows: The beginning of the coagulation was not marked, but at the end of half an hour the film was not thicker than common writing-paper; and this being removed, a little of the clear lymph was taken up with a wet tea-spoon, put into a clean cup, and was twenty minutes more in coagulating. Even at the end of an hour and an half, the whole of the blood was not jellied; for at this time I removed the film or pellicle, and took up a second portion of clear lymph with a spoon, and put it into a tea-cup, where it jellied afterwards; though this jelly was not indeed quite so firm as the *crassamentum* itself

EXPERIMENT XV.

A WOMAN, with a slight inflammation in her throat, had eight ounces of blood taken from her arm; the blood was
received

received into a basin, and the bleeding finished in four minutes and three quarters, when a film began to form near the air-bubbles ; in seven minutes a transparent size appeared over a considerable part of the surface which was quite fluid, whilst the rest of the blood was coagulating, there being now a very distinct red crust over the rest of the surface.

Now, from comparing these experiments with what has been observed of the coagulation of the blood, where there is no inflammatory crust or size, is it not evident that the blood remains longer fluid after being exposed to the air, and has less disposition to coagulate, in those cases where there is a size, than where there is none ? for where there was none, it was found to coagulate completely in seven minutes ; but in one of the others, where the size was very thick, it did not completely coagulate in less than an hour and an half,

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THE power that inflammation has in lessening the disposition of the lymph to coagulate is likewise plain from the following experiment, where the blood in the heart of a dead animal seems to have congealed very slowly.

EXPERIMENT XVI.

A DOG was killed eight hours after receiving a large wound in his neck. The wound had during this time inflamed considerably. Upon opening him next morning, when he had been dead thirteen hours, a large whitish *polypus*, was found in the right ventricle of his heart; under this was a little blood still fluid, which being taken up with a teaspoon, coagulated soon after being exposed to the air.

It may be proper to observe here, that in the hearts of animals which had died without any inflammation, I have found
the

the blood entirely coagulated long before this time. And that from opening them at different times, I have seen it coagulate in their hearts after death, in the same gradual manner that it does in their veins, when its motion is stopt by ligatures; as related in page 23.

IN the next place, that the blood is really attenuated in inflammatory disorders, where the whitish crust or size appears, is probable from the following circumstances: First, It even seems thinner to the eye; 2dly, The red particles or globules subside sooner in such blood, than in that of an animal in health. This seems proved by observing that in the above-mentioned experiments, where the blood was at rest in the veins, it was not covered with a crust, except in one or two instances, though in all those cases it remained longer fluid than the blood commonly does in a basin, after bleeding, where the crust appears. And again, the blood in the heart of an animal

mal that dies a violent death, is not generally covered with a white crust; notwithstanding it is so late in being congealed. These circumstances shew, that something more than merely a lessened disposition to coagulate is necessary for the forming of the crust or size. 3dly, The globules more readily subside in inflammatory cases, from the surface of the whole mass of blood, than they will afterwards do from the surface of a mixture with the *serum* alone, of which the following experiments are a proof. But, before I relate them, let me observe, that they were made with a view to discover, whether the inflammatory crust could be owing to any other cause than to the attenuation of the coagulable lymph, and to its disposition to coagulation being lessened: and as the same appearance might be suspected to arise from an increased specific gravity in the red particles, or from the *serum* alone being attenuated, I endeavoured to decide the question in the following manner.

E X P E-

EXPERIMENT XVII.

INTO a phial, marked A, I put an ounce of the *serum* of the blood of a person, whose *crassamentum* had an inflammatory crust.

INTO another, marked B, I poured an ounce of the *serum* of a person whose blood had no crust; then to each of these, I added a tea-spoonful of *serum*, loaded with the red particles of a person whose blood had no inflammatory crust or size. In attending to them, I could not observe that the red particles subsided at all sooner in the *serum* of the blood that had a crust, than they did in the *serum* of that blood which had no crust. Thence I conclude, that the *serum* is not attenuated in those cases where the inflammatory crust appears.

LASTLY,

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LASTLY, To see whether the specific gravity of the red globules was increased, I proceeded as follows :

EXPERIMENT XVIII.

I POURED into a phial C, a portion of the *serum* of the blood which had no crust ; and likewise into another D, a second portion of the same *serum*. I then added to C a tea-spoonful of the same *serum*, loaded with red particles from the blood which had an inflammatory crust. And into D I poured a tea-spoonful of the same *serum*, loaded with the globules of that blood which had no crust. In viewing these, I could not observe, that the globules of the blood which had an inflammatory crust subsided sooner than those of the blood which had none : whence I inferred, that the specific gravity of the red particles, or globules as they are called, is not increased in those cases

cases where the crust appears. And, therefore, since that inflammatory crust or size seems neither owing to the *serum's* being attenuated, nor to an increased specific gravity in the red particles, it probably depends solely upon a change in the coagulable lymph. And what seems farther to confirm this inference, in none of these experiments did the red particles subside from the surface of the *serum* in 20 minutes, though, where the crust appears, they subside from the surface of the blood in half that time; so that the whole mass of blood seems to be thinner than the *serum* alone; or, the coagulable lymph seems to be so much attenuated in these cases, as even to dilute the *serum*, which at first sight appears a paradox.

MAY we not, therefore, conclude, that in those cases where the inflammatory crust appears, the coagulable lymph is thinner, and its disposition to coagulation is lessened? both of which circumstances

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contribute to the subsiding of the red globules from the surface of the blood, which then coagulating gives rise to this appearance, called the inflammatory crust or size, in the blood of pleuritic or rheumatic patients *.

How contrary to the conclusion, which these experiments lead us to, are the opinions of some medical writers on this subject! How frequently do we find it said, that the blood is thicker in inflammatory disorders, where that size occurs; and that a larger orifice is necessary to let out the vitiated blood! That a large orifice is preferable to a small one in many cases, where such blood is found, I believe to be true; but that its advantages are owing to its letting out the thickened blood, seems improbable from what we

* This remarkable appearance might indeed be accounted for, by supposing that the lymph had ascended to the surface of the blood in those cases; but this is improbable, from considering, that, in its coagulated state, it is of greater specific gravity than the serum, and sinks in it.

have

have seen in the experiments above related ; they are perhaps nearer the truth, who attribute it to the suddenness of the evacuation.

IT may be proper to observe here, that this size or whitish crust is not a certain sign of inflammation ; it being often met with where there seems to be no such disease, in particular in the blood of pregnant women. And that it differs much in density in different cases ; in some it is extremely firm, in others it is spongy or cellular, and contains much *serum* in its cells. These diversities we shall endeavour to explain hereafter, when we have laid before the reader some more observations on the coagulation of the lymph †.

† Although this Essay has been so lately printed, yet most of the facts which occur in the preceding pages have been mentioned in my Anatomical Lectures, ever since the year 1767 ; and some of them were mentioned publicly even before that time. This I thought necessary to observe, because many of them have since appeared in other publications.



C H A P. III.

*Of the causes of the inflammatory crust's
appearing at different times in blood-
letting; of the stopping of hemorrhages;
and of the effects of cold upon the blood.*

IT has been observed by those who have written on the blood, that it sometimes happens in blood-letting, that the first cup has an inflammatory crust, whilst the last has none; but no satisfactory reason has been given for this difference. One might suppose that it was owing to some circumstance in the bleeding, such as in the different velocity with which the blood flowed into each cup, or to the last cup's being agitated so as to prevent the separation of the lymph: but I have seen it where there
was

was no difference of this sort, nor in any other circumstance that I could observe. I therefore suspect that in such cases the properties of the blood are changed, even during the time of the evacuation; to which opinion I was led by the following experiments.

EXPERIMENT XIX.

NINE ounces of blood were taken from a woman who had been delivered two days before, and who at that time laboured under a fever, with a considerable pain in her side, and in her *abdomen*. The blood was received into a basin, and her arm was tied up; when, on looking at the blood, I found its surface transparent for some depth, an indication of a future crust; and as her pain was not abated, and as her pulse could bear it well, I removed the ligature from her arm, and took away about six ounces more, into three tea-cups; but what

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appeared to me remarkable, although the blood flowed as fast into each of the cups as into the bason, and when full they were immediately set down on the same window, yet there was no inflammatory crust on the blood in the cups, though a very dense one on that in the bason. And again, although the blood in the bason had been taken away some minutes before that in the cups, yet it was later in being completely coagulated; as was evident on comparing them.

I HAD an opportunity of repeating the experiment in the evening; for the symptoms of inflammation seeming equally violent, it was thought proper by the physicians who attended her, to take away more blood; which was done by opening the same orifice, when three tea-cups were nearly filled, and set in the same place; and it was observed, that the first had a crust, though not so thick a one as in the first bleeding; but the
other

other two cups were without this appearance, though the blood had flowed into them even more quickly than into the first †.

EXPERIMENT XX.

A GENTLEMAN, who laboured under an inflammatory complaint, had about nine ounces of blood taken from his arm.

† As this experiment seems contradictory to some mentioned hereafter, in the last cups being filled rather sooner and yet coagulating sooner, which might be suspected to be owing to the vessel's acting more strongly at the latter part of the operation than at the beginning; it is therefore necessary to observe, that the difference in this experiment appeared to be only owing to a difference in the size of the orifice; for when the ligature was first removed, the old wound was not so much torn open as it was afterwards, when it was more enlarged in order to hasten the evacuation. But it did not, in the beginning of the operation, trickle down the arm as in Experiment 27; where the size of the orifice was not enlarged from the first, and yet in proportion as the operation advanced, the velocity of the blood increased; which was thence concluded to be owing to an increased action of the blood-vessels.

This quantity was divided into four portions; the first was received into a cup, and was in measure little more than an ounce; the second, into a bason, to the quantity of two ounces; the third into a cup, which held one ounce; and the fourth into a bason, to the quantity of three ounces. Each vessel was immediately placed upon the window; and it was observed that the blood in the first was latest in coagulating, and had a crust over the whole surface; that in the second had a crust only upon a part of its surface; but that in the third and fourth had none, and manifestly coagulated before either of the other two.

Now, since in these experiments the blood in the first cups was later in coagulating than that in the last; and since the blood in the first cups alone had a size, is it not probable, that even during the short time taken up in the evacuation, the properties of the lymph had been changed,

changed, and that it was owing to this change that the size disappeared? It might indeed, at first sight, seem possible, that the bleeding had only let out the vitiated part; but this is not at all likely; for, suppose a part only of the blood was vitiated, that part must have been equally diffused through the whole mass, and there is no probability of its getting out of the vessels before the rest of the blood; and consequently it ought to have appeared in the last equally as in the first cup, but it did not. Bleeding, therefore, in those cases alters the nature of the blood, not by removing the vitiated part, and giving room for new blood to be formed, as has been supposed; but probably by changing that state of the blood-vessels, on which the thinness, and lessened tendency of the lymph to coagulation, depends; which surely is a very curious circumstance ‡.

FROM

‡ That the properties of the blood can be changed by emptying the blood-vessels, is likewise proved by
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FROM this observation we may be led to think, that it may be useful to receive the blood more frequently into small cups, instead of a basin, and to attend more carefully to the alteration produced upon it by bleeding; as we may by that means perhaps learn to determine better, what quantities should be taken away in particular cases. For it would seem probable, that the operation is likely to have the most effect on the disease, in those cases where the greatest change is produced by its means, on the disposition of the blood to coagulate; and of that change, we can judge, by comparing the blood in the first cup, with that in

an experiment hereafter to be related; where the blood in an animal in health was found to have its disposition to coagulation increased, in proportion as the vessels were emptied, and as the animal became weaker. It may likewise be proper to mention, that though the inference is here drawn from two experiments only, yet I have likewise observed the same appearance in other cases, which I have thought unnecessary to relate.

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the last; for the first cup will nearly shew the state of the blood at the beginning; and the last cup the state of the blood at the latter part of the evacuation.

IT frequently happens, that instead of an inflammatory crust over the whole surface of the *crassamentum*, there is only a partial one, which appears in large spots or streaks. In such cases I have observed, that only a part of the blood had its disposition to coagulate lessened, as in Experiment XV. in which some of the blood remained fluid and transparent, where those streaks appeared, for some time after the coagulation had begun in other parts of the surface. Now whether in those cases there had been the same difference before the vein was opened, or whether the whole blood had not been of the inflammatory kind, before venesection, and a part of it was changed as it ran out, or as soon as the general fulness was diminished, may be a question; but the probability, I think,
is

is much in favour of its being changed during the time of the évacuation, from what was observed in the last experiments.

WHEN I had observed that this disposition of the lymph to coagulate was increased by bleeding, or by weakening the action of the blood-vessels, I suspected that possibly in those cases where the body was very weak, the disposition to coagulate might be so much increased, that instead of being three or four minutes in beginning to do it, after it is let out of the veins (as is the case in people in health) it might coagulate in less time, or almost instantaneously; for I imagined, that unless this took place, we could hardly conceive how the blood should ever have time to coagulate in ruptured vessels, so as to stop hemorrhages, as it is believed to do. And upon this occasion I recollected a remark that I had heard, particularly from Dr. Hunter, which

which is, " That the faintness which
" comes on after hemorrhages, instead
" of alarming the bye-standers, and
" making them support the patient by
" stimulating medicines, as spirits of
" hartshorn and cordials, should be
" looked upon as salutary; as it seems
" to be the method Nature takes to give
" the blood time to coagulate." Now
as this seemed to favour my suspicion, I
determined to make the experiment.

EXPERIMENT XXI.

BELIEVING it would be sufficient for
this purpose, to attend to the properties
of the blood, as it flows at different times
from an animal that is bleeding to death,
I therefore went to the markets, and at-
tended the killing of sheep; and having
received the blood into cups, I found my
notion verified. For I observed, that the
blood which came from the vessels imme-
diately

diately on withdrawing the knife, was about two minutes in beginning to coagulate; and that the blood taken later, or as the animal became weaker, coagulated in less and less time; till at last, when the animal became very weak, the blood, though quite fluid as it came from the vessels, yet had hardly been received into the cup before it congealed. I have also repeated the experiment, by receiving blood into different cups at different times, whilst the animal was bleeding to death; and though the time taken up in killing the animal was not commonly more than two minutes, yet I observed, on comparing the cups, that the blood which issued last coagulated first*. I have observed likewise,

* It may be necessary to mention a circumstance that has occurred in repeating these experiments; which is, that although the last cup being taken from the animal when much reduced, always coagulated in less time than the first; yet when four or five cups were used, the blood in them did not always coagulate precisely in the inverse order of their being filled; for sometimes the second coagulated

likewise, that the blood coagulates with a different appearance in proportion as the animal becomes weaker; that which follows the knife begins to coagulate in about two minutes; it first forms a film or pellicle on the surface, which extends gradually through the whole blood, yet so slowly that its progress may be observed, especially if the pellicle be moved from time to time. But the blood that comes from the fainting animal is coagulated in an instant, after it once begins. From this circumstance, that the disposition of the blood to coagulate is increased as the animal becomes weaker, we may draw an inference of some use with regard to the stopping of hemorrhages, viz. not to rouse the patient by

lated before the third. This circumstance at first seemed contradictory to the general conclusion, but on a more careful examination, it was suspected to be owing to the struggles (or temporary exertion of strength of the vessels) of the animal, and no difference was observed in the exposition to cold or to air.

stimulating

stimulating medicines, nor by motion, but to let that languor or faintness continue, since it is so favourable for that purpose; and also, that the medicines likely to be of service in those cases, are such as cool the body, lessen the force of the circulation, and increase that languor or faintness*. For, in proportion as these effects are produced, the divided arteries become more capable of contracting, and the blood more readily coagulates; two circumstances that seem to concur in closing the bleeding orifices†.

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* Besides giving stimulants and cordials to counteract the fainting, it is a common practice in many parts of England, to give women, who are flooding, considerable quantities of port-wine, on a supposition that it will do them service by its astringency. But surely, from its increasing the force of the circulation, it must be prejudicial in those cases. Perhaps many of the remedies called styptics might be objected to for the same reason.

† It has of late been proved by experiments, particularly by those of the ingenious Mr. Kirkland, that the larger arteries, when divided, contract so as
to

It has been questioned whether blood-letting can be properly recommended in hemorrhages, excepting in those that are attended with evident signs of *pletbora*: but do not these experiments shew, that a vein may be opened with propriety, even where there is no *pletbora*, in order suddenly to bring on weakness; by which the momentum of the blood may be so diminished, and the disposition of the lymph to coagulate may be so increased, as to stop the hemorrhage? For, when we consider how soon the blood-vessels contract, and adapt themselves to the quantity of blood which they contain, it seems

to stop the hemorrhage. But the large *coagula* which we see in the orifices of the vessels of the *uterus* of those who die soon after delivery, and the stopping of hemorrhages where the blood-vessels were ruptured on their sides and not entirely divided, make me believe that contracting the bleeding orifice is not the only method nature takes to stop an hemorrhage. Her resources indeed are great, and she has often more methods than one of producing the same effect.

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not

not improbable that in some cases where the hemorrhage is not profuse, but long-continued, the strength of the patient may be so recruited, that the disposition to coagulate shall not be sufficiently increased, or the extremities of the vessels sufficiently contracted, for the stopping of the bleeding; but, by emptying the vessels suddenly, this effect may be obtained, and the hemorrhage may be stopt by the loss of less blood, than would have happened, had only the slow draining been continued.

ALTHOUGH the whitish crust so commonly seen in inflammatory disorders, has so very morbid an aspect, as might induce us to consider it as inflammatory, and to bleed repeatedly in all those cases where it occurs, yet I believe we should act improperly: for, to say nothing of pregnancy, in which the appearance is almost constant, there are few physicians that have not seen patients, who, even
in

in such circumstances, were the worse for this evacuation. Nor need we be surprized that this should happen, considering how soon in some instances this size disappears; and if so, may we not suppose, that it may likewise soon be formed, even by a short exertion of strength in the vessels? Perhaps this was the case in the gentleman mentioned in page 55, who in less than twenty-four hours after bleeding, had symptoms of great weakness.

As it appears from these experiments, that the disposition of the blood to coagulate is increased by bleeding, it may be useful to attend more to this circumstance, and to compare the coagulation of the blood in the last, with that in the first cup, even in cases that are not attended with the inflammatory crust. And it may likewise be worth while to make the same comparison in those cases where every cup has a crust; which frequently

quently happens both in rheumatic and in phthifical complaints. By these means we may judge what effect the evacuation has produced on the strength or fulness of the vessels ; and may perhaps, by inspecting the last cup, especially if it contains only a small quantity, be able to guess pretty nearly at the nature of the blood which remains in the body. In the rheumatic case mentioned in page 41, every cup contained this crust ; and although the blood in the last cup coagulated in much less time than that in the first, yet as it was later in coagulating than common, I suspected what remained in the vessels had the same disposition ; but the patient recovered without repeating the evacuation.

It may be mentioned here, that I have once or twice seen blood, which, when it first began to coagulate, had on its surface a red pellicle, and underneath a transparent fluid, which afterwards
formed

formed a crust. In these cases, if the red pellicle had not been removed before the rest of the blood had congealed, we might have concluded that no part of the blood had this disposition to form a white crust. This appearance, I should imagine, was owing to the blood, where in contact with the air, having coagulated before the red particles had time to subside, from that part of the lymph which had its disposition to coagulation lessened.

THE learned professor de Haen has taken notice of a curious appearance of the blood, which he could not account for ; but which, I presume, may be explained from some of the above experiments. His observation is, “ that, “ having bled a person in a fever, the “ blood was covered with an inflamma- “ tory crust, and upon examining the “ *crassamentum* in one of the cups, he “ found that it formed a sort of sack

F 3

“ contain-

“ containing a clear fluid: this fluid
 “ being let out, and the cup set by, on
 “ examining it next morning, he ob-
 “ served a very firm crust covering the
 “ whole again, and extending to the
 “ bottom of the cup *.” I once met
 with a case similar to this; for, having
 bled a person into four cups at ten
 o'clock in the morning, on looking at
 the blood afterwards, at five in the af-
 ternoon, I found the *serum* had not sepa-
 rated from the *crassamentum* in the first
 cup; but the *crassamentum* felt as if it
 contained a fluid in a bag, as professor
 de Haen has described it. Upon pres-
 sing it, the fluid gushed out, and in a
 few minutes after being exposed to the
 air, coagulated: there was however this
 difference in the two cases, that in mine
 the fluid was red, so that it formed a
 red crust over the first, which was white.
 Now this seems to have been owing to the
 blood's having first coagulated, where it

* Vide Rat. Medendi, cap. vi.

was in contact with the air and with the sides of the cup; and the fluid which gushed out was the *serum*, with a part of the coagulable lymph, which still remained fluid; but, when exposed to the air, it jellied or coagulated, as it naturally does. That one part of the lymph can remain fluid after the other is coagulated, is proved by some of the preceding experiments; and I have more than once seen blood, which appeared perfectly jellied soon after bleeding; yet, on cutting into the *coagulum*, a transparent fluid has oozed out, which afterwards jellied. And so slowly does this coagulation proceed in some cases, that in an experiment mentioned before, a part of the blood in a dog's heart was found uncoagulated thirteen hours after death. And I have likewise distinctly observed, that in some cases where the disposition to coagulate was much lessened during the evacuation, the blood at the bottom of the cup has jellied, whilst the greatest part of the *size* at the top was

yet fluid ; there being only a thin pellicle on its surface, where it was in contact with the air.

ANOTHER instance of a change in the properties of this coagulable lymph, which appears curious, was seen in some experiments, where I had occasion to throw the blood into water, and into oil, during the winter season, whilst the heat of the water and of the oil was no greater than 41° of Fahrenheit's scale. In all those experiments, I found that the disposition to coagulate was lessened, the blood becoming more and more viscid, but did not coagulate whilst in that degree of cold. I shall next relate those experiments.

EXPERIMENT XXII.

THE jugular vein being properly tied, and then cut out from a rabbit just killed, was thrown into water of 41° of heat,
and

and taken out at the end of half an hour; when the blood was found to be still fluid, though rather more viscid than natural; but, after being exposed to the air, it coagulated.

EXPERIMENT XXIII.

Two pieces of the jugular vein of a dog, just killed, were put into water, in which the thermometer stood at 41° ; one was taken out after twenty minutes, and the other after three quarters of an hour; the blood in both was found to be fluid, and to coagulate afterwards.

As it was evident from these experiments, that the water had lessened the disposition of the blood to coagulate, I next enquired to what property in the water this effect could be owing; and to see whether water that was warmer would not have the same effect, I made the following experiment.

EXPE-

E X P E R I M E N T XXIV.

ON December the thirteenth, I cut out two pieces of the jugular vein of another dog, immediately after his death. One piece was put into cold water, and the other into water kept warm by a lamp, so that the heat never varied more than between 90 and 100°. At the end of three quarters of an hour, that in the warm water had in it a *coagulum* as large as a garden-pea; but that in the cold water, being let out into a cup, was quite fluid. Twenty minutes after being exposed to the air, that which had been in the cold water was coagulating; but that from the warm water neither then nor afterwards shewed any signs of farther coagulation: so that it seemed not only to have jellied whilst in warm water, but to have begun to part with its *serum*. From this experiment, it seems probable that the *coldness* was that property

property of the water to which the lessened disposition to coagulate was owing; but, to be more sure of this, and to see whether the blood might not be kept fluid a longer time by these means, I tried as follows:

EXPERIMENT XXV.

ON January the fourteenth, I cut out a piece of the jugular vein of another dog, and put it into oil, in which the thermometer stood at 38° . At the end of six hours it was taken out, and the red particles were observed through the coats of the vein to have mostly settled to one side. The blood was let out into a cup, and was found to be fluid; at the end of fifteen minutes above one half was still fluid; in twenty-five minutes it seemed to be quite jellied. Now as in this experiment a similar effect was produced, as when the vein was put into water, it seems probable that it was the coldness

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coldness of the water, and of the oil, which had lessened the disposition of the lymph to coagulate.

EXPERIMENT XXVI.†

ANOTHER piece of the same vein was put into river-water, in which the thermometer stood at 38° , and was left till the next morning; when, after twenty-two hours and a quarter, it was taken out. The red particles did not seem to have subsided, as in the former experiment; but the vein being opened the blood was found to be fluid, though so viscid that it could barely drop from the vessel. The cup into which it was received was placed upon the window of a

† It is necessary to observe here, that great expedition should be used in making these experiments; for, unless the vein be cut out in a few minutes after the death of the animal, the experiment may not succeed, from the blood's having begun to coagulate.

moderately

moderately warm room, and was examined carefully from time to time; but the blood never had any appearance of coagulation, on the contrary, it remained fluid till it was dried by the evaporation of the water, which happened by the next day. In this experiment the cold seemed entirely to have prevented the coagulation of the lymph: so ill-founded is the common opinion, that cold coagulates the blood.

As the lymph, on being cooled, is deprived of its power of coagulating when exposed to the air, may we not thence be led to explain that fact mentioned by Lister, that the blood of those cold animals which sleep during the winter-season, on being let out into a basin, does not coagulate? And thence, as he observes, remains always fit for motion.



C H A P. IV.

Some further observations on the coagulable lymph, and on the sudden changes produced upon it.

IF the reader has been persuaded of the common opinion, that the disposition of the blood to coagulate is increased in inflammatory disorders, it may perhaps appear to him, as it formerly did to me, a very extraordinary circumstance that the contrary should be true ; and likewise that the blood should in reality be the more disposed to concrete, in proportion as the body is weakened, or as the action of the blood-vessels is diminished. And as we are naturally tenacious of old opinions, and unwilling to adopt new ones
till

till fully proved, he may suspect that there has been some fallacy in these experiments. And indeed I must acknowledge that there is, in appearance, one strong argument against my general conclusion, which is, that it has not only been remarked, that the first cup has a crust, whilst the last has none; but likewise, that the second, or the third cup, alone shall have a crust, whilst the preceding ones are without it. Now this, I say, seems contradictory to what I have advanced, concerning the disposition of the blood to coagulate being increased in proportion as the body is weakened; for here in proportion as the blood is evacuated, its disposition to coagulate is lessened; since it was more fizy in the second, or third cup, than in the first. But, in answer to this objection, I must remark, that these cases very seldom occur; and that in general the first cups are more fizy, and are the latest in jellying; and when the contrary takes place,

place, or when the second or third cup is more fizy than the preceding, I am persuaded, that upon a careful examination, instead of weakening, they will be found to strengthen my inference; as will appear probable by the following case, which has occurred since these experiments were published in the Philosophical Transactions.

E X P E R I M E N T XXVII.

ON the 13th of June, I visited a young man, twenty-two years old, of an athletic habit, who complained of a violent pain in his head and back, with a full strong pulse; but as he was then in a profuse sweat, which had been preceded by a shivering, it was not thought proper to bleed him, and the rather, as we were informed, that he had had a similar paroxysm two days before. But next day, finding that his fever had not left him with the sweat, and that he still had
a pain

a pain in his head and back, and that his pulse, though not now full and strong, yet was quicker than natural, it was then judged necessary to take away some blood. Upon opening the vein, the blood flowed very slowly, and indeed merely trickled down his arm. Imagining that the bandage might be too tight, I slackened it, but still the motion of the blood was not accelerated. I then asked him whether he had not been afraid of the bleeding, and he told me he had; and on feeling his pulse in the other arm, I found it very low. I therefore desired him to move the muscles of his hand, which he did; but nevertheless so slowly did the blood run, that it was four minutes before I got an ounce and an half into a cup. I then stopt the orifice till another cup was brought, into which the blood ran in a full stream, to the quantity of three ounces, and that in two minutes, although the orifice was rather small, so much was its velocity now increased.

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creased. Into the third cup, which likewise held three ounces, the blood ran still faster, as it was filled in less than two minutes. By this time the patient beginning to be faint, I stopt the bleeding till he could lie down on the floor, and then about three drachms more of blood were received into a fourth cup: this came away very slowly, and the bleeding stopt of itself. He drank a glass of water, and did not faint, and he appeared afterwards to be much relieved by the evacuation. Upon this blood I made the following remarks:

THAT which was taken away last was first coagulated, and completely too, by the time I had tied up his arm, which was in three minutes from the blood's first running into the cup.

THE blood which was received into the first cup coagulated next, and as I observed by my watch, in twelve minutes from its being set down on the table.

THAT

THAT which was received into the second cup was the third in order as to coagulation, and was considerably later in jellying than the first; for in fifteen minutes it was not thoroughly coagulated; nay, even in twenty-two minutes a small part of it was still fluid. It was remarkable, that none of these three had any size.

BUT the blood in the third cup differed considerably from that in the others; for in five minutes it began to appear transparent on its surface, an indication of a future size, and it was later in coagulating than that in the other cups; for even at the end of twenty-six minutes a great part of the coagulable lymph was still fluid, as appeared on removing the pellicle that covered it; but in thirty-five minutes it was completely jellied. The size in this blood was very thick and tough.

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Now this case, when carefully examined, instead of being an objection to my conclusions, will, I presume, be thought a strong confirmation of them.

FOR, in the first place, as the blood in the third cup alone had a crust, and was much later in jellying than the rest, it strengthens my inference, that the disposition of the blood to coagulate is lessened in those cases where the inflammatory crust or size appears. And as the blood ran more rapidly into this cup, it shewed that the heart and blood-vessels had begun to act with greater force, and therefore confirmed the opinion, that in proportion as these act more strongly, the disposition of the lymph to coagulate is diminished. The same opinion is likewise supported by observing what happened to the blood in the first cup, which coagulated sooner than that in the third, owing to the vessels then acting more weakly,

weakly, as was evident from the blood's trickling down the arm, and from the lowness of the pulse*.

2dly, It may be observed, that the great difficulty in admitting the conclusion made in the former part of these

* In like manner may be explained another variety in the appearance of the size, namely where it is found in the first and last cups, but not in the second or third: this I suspect seldom happens, but when it does, it may perhaps be found, on examination, that the vessels were acting more weakly whilst the second or third cups were filled. For, so easily does this size appear to be removed, or formed, that I suspect it may sometimes happen, that when the blood is taken away, in a full stream, from a large orifice, the patient may be so suddenly weakened, and the properties of the blood may in consequence be so changed by the time the second cup is filled, that the size shall be removed: and yet afterwards the vessels may recover their former tone, so that the third or fourth cup may acquire a size again. Nay, I suspect that this appearance may even be affected by the passions, particularly from observing that the patient abovementioned, as well as others whose blood at first trickled slowly down their arms, had been much afraid of the lancet.

sheets (viz. that the want of size in the last cup is occasioned by an alteration in the blood-vessels) was to conceive how these vessels could possibly alter the properties of the lymph so suddenly, as in the time between receiving the blood into the first cup, and into the last. But this case confirms that inference, by shewing the fact in a clearer point of view; for even here, where the appearance of the size was reversed, it was found that the blood which had a crust or size was latest in coagulating, and that it was this blood which was taken out of the vessels when they acted most strongly, as was proved by the rapidity with which it flowed into the cup.

3^{dly}. SINCE the times in which the blood jellied in these cups were so very different (the first coagulating in twelve minutes, the second in about twenty-two, the third in thirty-five, and the fourth in less than three minutes, notwithstanding these

these cups were filled in less than two minutes after one another) it shews, I say, how soon that state of the blood-vessels on which the size depends, can be removed and assumed, and therefore leads us to conclude, that although this size is in general a sign of an inflammatory disorder, or a strong action of the vessels, yet there may be several circumstances to be taken into the account, before we can judge from its presence, or absence, whether or no venesection should be repeated: and it likewise shews clearly, that it would be improper to determine, from the presence of this alone, when bleeding is necessary; and yet there have been not a few who have inclined to make such a conclusion, from their considering this crust or size as so very morbid an appearance.

4^{thly}, As the blood in the third cup was so late as thirty-five minutes in coagulating, and was fizy, whilst that in the

fourth was not so, and jellied in less than three minutes, although it had been taken from the vessels only two minutes after the other, but at the time the patient had become faint; it shews how much faintness and languor increase the viscosity of the blood, and likewise its disposition to coagulate, since in two minutes they produced such a change as to remove the size, and to reduce the time of coagulation from thirty-five to three minutes. It therefore shews clearly how much languor and faintness should be encouraged in hemorrhages, and how carefully we should avoid giving any thing that can stimulate, or rouse the patient; that the medicines likely to be of service are nitre and the acids; or such as cool the body, or have the property of diminishing the force of the circulation, or of increasing that languor or faintness*; that

* It has been objected here, that nitre would seem improper for this purpose, because in experiments

that all agitation of mind should, as much as possible, be prevented, lest it increase the circulation: that all muscular motion should be avoided for the same reason: for that an exertion of the patient's strength can lessen the disposition of the blood to coagulate, I am persuaded from some of the abovementioned cases, and likewise from what I have observed in dying sheep, where the struggles of the expiring animal seemed in some instances,

ments mentioned before (p. 12.), it was found to prevent the coagulation of the blood, out of the body; but this objection is removed, by considering, that, in order to prevent coagulation, the nitre must be used at least in the proportion of two scruples to every two ounces of blood. But, when we exhibit it internally, we seldom give more than a scruple every two hours, which can have no effect in attenuating the whole mass of blood, nor in preventing coagulation; especially as we have reason to believe its properties are changed, before it passes the digestive organs. Its good effects in hemorrhages, therefore, are probably owing to its action upon the stomach. For proofs of its utility, see *Medical Observations and Inquiries*, vol. IV. art. xvi.

when

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when violent, to alter the properties of
the lymph.

WE have endeavoured to explain the appearance of the inflammatory crust or size, from the red globules having subsided from the surface of such blood before it coagulated: this we observed was partly owing to the lymph's being later in coagulating in those cases, but principally to its being thinned. But we may now add, that although the attenuation of the lymph, and its lessened tendency to coagulate, are connected in most of those cases, yet they do not always go together; for the lymph may have its disposition to coagulate lessened without being thinned; which was evident in the preceding case, on comparing the blood in the second with that in the third cup; for the blood in the second cup had no size, notwithstanding it remained fluid at least ten minutes after the size had begun to appear in the third: this I attribute to the blood in the third being

being more attenuated, and thereby more readily allowing the globules to subside.

THAT the blood may have its disposition to coagulate lessened, without being attenuated, is likewise probable from the following cases.

EXPERIMENT XXVIII.

IN the month of January I bled a man, who complained of a pain in his head, attended with giddiness and shivering, a pain and sickness at his stomach, and with a full and quick pulse: the blood was found to remain fluid for ten minutes, and then jellied, but no size appeared.

EXPERIMENT XXIX.

IN another person, who was bled merely for a drowsiness, and because he was accustomed to that evacuation in the Spring,

Spring, I found the blood remain seven minutes without coagulating, and yet it was without any fize.

Now, since in these cases the blood remained so long fluid, and yet the red particles did not subside, or no fize appeared, I should conclude, that only the disposition of the lymph to coagulate was lessened, without its being thinned. And from the last case we may likewise conclude that although the times, at which the blood taken from persons in health begins to coagulate, be allowed to be about three minutes and an half, as I have found from repeated observations, yet there may be some variety in this respect; for a *plethora* and other circumstances may make it later in coagulating in some cases, even where the patient is otherwise in perfect health *.

* This inference is confirmed by a case mentioned below, Experiment 32.

WE have observed before, that the size is sometimes very firm, and at other times spongy and cellular; these differences in its density are, I suspect, in proportion to the degree of attenuation and lessened disposition of the blood to coagulate; for as the coagulation begins on the surface, and forms there a film which attracts the rest of the lymph, the more that lymph is attenuated, and the slower it coagulates, the more will the film be able to separate it from the red globules, and from the serum: thence perhaps it is, that when the blood, besides being very thin, likewise jellies slowly, we sometimes see almost the whole coagulable lymph collected at the top, forming a firm crust, which being free from the serum, as well as from the globules, contracts the surface into a hollow form. But when the blood has its disposition to coagulate less diminished in proportion to the attenuation, then, although the globules subside from the surface, yet the whole

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whole of the lymph jellies so soon after the coagulation begins, that there is not time for its being separated from the serum, of which it therefore contains a considerable quantity, and is of course more spongy and cellular.

IN proportion to the thickness and density of the size, the bottom of the cake is of a looser texture; but this looseness of texture is not owing to putrefaction, as has been suspected, but merely to the lymph's being collected at the top, and therefore leaving the bottom of the *crassamentum*.

NOTWITHSTANDING bleeding does in general weaken the action of the vessels, increase the disposition of the blood to coagulate, and even thicken the lymph; yet it may happen, that, in the ordinary quantity in which blood is taken away, none of these effects shall be produced; of this the following case seems to be an instance.

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EXPERIMENT XXX.

A WOMAN in the seventh month of her pregnancy was bled for a violent pain in her side, attended with a cough ; the quantity taken away was eight ounces, which was received into four cups ; and as the orifice was small, about ten minutes were spent in the bleeding. On attending to the different cups, I could observe no difference in the periods at which the coagulation commenced, and finished in each, allowance being made for the time the blood began to run into each. In every one of these cups the blood was completely jellied in about twenty minutes, and each had a crust or size nearly of the same thickness. So that the bleeding seemed not to have produced any change in the strength of the patient's vessels, nor was her pain sensibly abated by it. She was therefore desired to live low, to confine herself to
a vege-

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a vegetable diet, and to take a scruple of nitre every three hours in a draught of the *decoctum pectorale*; and if her pain and cough were not abated in a day or two, she was directed to repeat the bleeding. As close attendance was not required, I did not visit her till four days after, and then she had got free of her complaints, notwithstanding her blood had been apparently so little changed in the time of the evacuation.

IN this case the bleeding seemed neither to have thickened the lymph, nor increased its disposition to coagulate, nor weakened the action of the vessels; but that it generally produces these effects, cannot, I think, be doubted, from our having observed it in so many instances. Perhaps the dread of the operation might here have made the coagulation of the blood in the first cup approach nearer to that in the last; or perhaps the smallness of the orifice prevented there being so manifest

manifest a change produced by the evacuation, from its giving time to the blood-vessels to adapt themselves more equally to the quantity they contained, by which means she was not weakened by the loss of blood.

It has been observed by Sydenham and others, that it sometimes happens, even in inflammatory disorders, when the blood trickles down the arm, instead of running in a full stream, it does not acquire a crust or size *. May not this be

* It may be necessary to observe, that it is not in every case where the blood trickles down the arm that it is without a size; on the contrary, it sometimes happens, that even in such circumstances it has a very dense one; an instance of which may be seen below, in Experiment xxxi. In those cases the trickling down the arm may perhaps be owing to some circumstance in the orifice preventing its flowing in a full stream, or to a difference in the tightness of the ligature, rather than to a weak action of the vessels. Or, although the size be occasioned by a strong (or some particular mode of)

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action

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be explained from what is observed in the case related in Experiment xxvii? that is, in such instances the vessels, either from a febrile, or from some other oppression, act more weakly than they do in the ordinary cases of inflammation; by which means the lymph is not sufficiently attenuated to allow the red globules to subside before the coagulation begins, and therefore the size does not appear, as in other cases of inflammation where there is no such oppression.

As air is found to coagulate the blood and cold to thicken it, an objection has thence been made to the conclusions from some of the preceding experiments; and

action of the vessels, and therefore is removed by weakening them, yet it may not always be removed immediately on their being weakened. For it may happen, that in some cases the lymph may not be so susceptible of changes as in others; or when it has been very much attenuated it may not again be thickened immediately, on the vessels acting weakly.

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it has been supposed, that the changes in the properties of the blood, that happen during the time of bleeding (which I have attributed to a difference in the action of the vessels) might possibly be owing merely to a difference in the exposition to the air, or to cold. For instance, since the blood that trickles down the arm seems to be more cooled than that which flows in a full stream, it has thence been supposed, that its want of size, in those cases, might be owing to the exposition to the air, which made it coagulate sooner, and to the cooling which had thickened it, and thereby prevented its red particles from subsiding so that the size should be formed. This objection is indeed plausible, and to those who have not seen these experiments, might at first seem sufficient to explain the appearance; but upon further examination it will not be found to do it satisfactorily. Thus, for example, although it be true that air coagulates the blood, and likewise cools it, yet

there are changes remarked in the preceding experiments that cannot be explained merely by a difference in the exposition to the air: Thus, in Experiment xxvii. the blood in the third cup was thirty-five minutes in being completely coagulated, whilst that in the fourth, although taken from the arm only two minutes later, yet coagulated in three minutes. Now no exposition to the air, nor to cold, from the blood's trickling down the arm, could produce such a change. Of this I am persuaded from what I have observed on comparing the blood received into a cup, with that which dropt on the plate which held the cup; for I have repeatedly seen on those occasions, that the blood on the plate, although it was so much more cooled and so much more exposed to the air, than that in the cup, yet instead of coagulating proportionably sooner, was later in being coagulated. The following experiment shews this clearly.

EXPE-

EXPERIMENT XXXL

A YOUNG woman with a violent inflammation in her eyes, was bled on the 5th of March, early in the morning, before she had breakfasted, and whilst she was complaining of a sickness at her stomach; the blood followed the lancet in a stream, but immediately after it only trickled down the arm, and continued to do so during the whole of the evacuation. About eight ounces of blood were taken away into four vessels, viz. into two cups and two saucers, in the following manner: A plate holding both a cup and a saucer was held under the arm, and the blood was first received into the saucer, to the quantity of a spoonful, then as much more was received into the cup that stood by it; then again the blood was suffered to run into the saucer, and afterwards into the cup, and so alternately till there was

about two ounces in each, when they were carefully set down on a window where the thermometer stood at 57° ; the plate was placed by them, and contained about a spoonful of blood, which had missed the saucer in the beginning of the evacuation. Next, the second plate was brought, and some blood was received first into the cup and then into the saucer, in the same manner; and three portions of blood were suffered to drop at different times on the plate, each of them about the breadth of a shilling. Now, here, according to the reasoning in the objections made to some of the preceding experiments, the blood in the saucers having twice as much surface as that in the cups, ought to have coagulated in half the time; and that on the plates ought, from the largeness of the surface to have coagulated in much less time; but just the contrary happened; for the blood in the cups was first completely coagulated, that in the saucers next, and that on the plates latest of all. But,
as

as the experiment seems curious, it may be proper to give a more particular detail of what was observed.

ON looking at the first plate, at the end of seven minutes after it was set down on the window, the surface of the blood in the cup was considerably transparent, and a pellicle (that is the surface beginning to coagulate) was formed upon it; but no transparency was distinguishable on that in the saucer, nor could any pellicle be observed upon drawing a pin through it, or through that which had dropt upon the plate. At the end of fifteen minutes the blood in the cup and in the saucer were pretty much coagulated, or had a thick pellicle, whilst none could yet be observed on that upon the plate. At the end of fifty-five minutes that in the cup was just beginning to part with its serum, whilst the blood in the saucer was not yet completely coagulated; for on inclin-

ing it to one side, a part of the blood appeared fluid under the pellicle. That on the plate was now coagulated. They were all three fizy; and the blood in the faucer had a size which seemed to be as firm, and in as large a quantity, nearly, as that in the cup; and the size upon the blood in the plate was thick enough to be easily distinguished.

IN the second plate, at the end of seven minutes after being set on the window, both the blood in the cup and in the faucer were beginning to coagulate; and had a pellicle of a considerable thickness, and were both fizy: but no pellicle appeared on any of the three portions that had dropt on the plate. At the end of fifteen minutes, that in the cup was firmly jellied, that in the faucer not quite so much, and one of the spots on the plate was but just beginning to coagulate at its edge. At the end of twenty-five minutes, the two last spots were

were still perfectly fluid, but in twenty-eight minutes they were beginning to coagulate; whilst the blood in the cup was now parting with its serum. At the end of fifty minutes a considerable quantity of serum had separated in the cup, and the separation was just beginning in the faucer.

THIS experiment was repeated on another person's blood two days after, in the presence of Mr. Field and Mr. Hendy, two studious gentlemen, at that time living at the Middlesex Hospital, and the appearances were exactly similar; and it was evident to them that the blood in the faucers was later in coagulating than that in the cups, and that on the plates (one of which was of pewter) was considerably later in jellying than that in the cups or in the faucers.

THESE experiments therefore shew clearly, that the differences in the periods

riods of coagulation, and in the appearance of size upon the blood received into the different cups in bleeding, cannot be accounted for from a difference in the exposition to air; for here blood more exposed to the air than that is which trickles down the arm, is found equally sized, and to be even later in coagulating than blood less exposed.

As we have here observed a new circumstance that appears remarkable, and which at first sight seems not reconcilable to some of our conclusions, it may therefore be necessary to examine it farther, or to enquire, If air be a coagulant of the blood (as we have endeavoured to prove in the beginning of this Essay) how comes it that in this experiment the blood was not coagulated proportionably to its exposition to the air? This, I think, may be explained from considering another fact that was mentioned in the preceding pages, *viz.* that
cold

cold lessens the blood's tendency to coagulation. The blood, therefore, in the saucer, although it was more exposed to the air, yet being more cooled than that in the cup, was, for that reason, later in coagulating; and that on the plate, which was most exposed to the air, being most cooled, was therefore latest of all.

BUT we may add, that although it be evident from this experiment, that the difference in the exposition to air, or to cold, is not sufficient to explain the changes which we see produced upon the blood, in so little time as in the filling of a small cup, especially when those changes are so great as what are mentioned in Experiment XXVII (where the blood in one cup was thirty-five minutes in coagulating, and had a very thick size, whilst that taken away soon after, coagulated in three minutes, and was without a size) yet, I think, that the effects of air, and of cold, are considerable enough to deserve to be

be taken into the account in some cases, where the changes on the blood are not so great. For as cold thickens the blood, it is probable that in some cases where the lymph is but little attenuated, and where therefore, in the ordinary manner of bleeding, there would have appeared but very little size, such blood, if more exposed to the air so as to be sooner cooled, may thereby have the small degree of attenuation counteracted, or removed, and the red particles may be prevented from subsiding. So that although in this experiment, where the size was thick, it appeared equally in the saucer as in the cup, and even appeared on the blood upon the plate; yet, if we repeat these experiments on a variety of subjects, it is probable, that we may sometimes find the saucer without a size, whilst the cup has one; for when the lymph is but little attenuated a slight cause may thicken it again; and its being a little more cooled in the saucer
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and on the plate, may in some cases be sufficient to prevent the size from appearing.

It has likewise been suggested, that possibly there might be some difference in the orifice, from which the blood flowed, to which its different appearances in the several cups might be owing. But there does not seem to be any foundation for this objection, and it may, I think, be removed by a careful examination only of some of these experiments, particularly the 27th; for there the blood ran in a full stream both into the second cup and into the third, the orifice being apparently unaltered, and yet there was a great difference in the appearance of the blood: for that in the third cup had a thick size, but that in the second had none. So that there does not seem to be any circumstance attending these experiments that can explain the changes produced upon the blood

blood in the time of bleeding, excepting that to which I have attributed it, *viz.* a change in the strength of the blood-vessels, or in their mode of action; and every observation I have yet made confirms me in that opinion.

A VERY eminent physician †, after reading the first edition of these sheets, informed me, that from a suggestion which he met with in Professor Simson's Differtations, *viz.* "That a ligature on the arm would produce a size †," he had been for many years cautious how he took any indications from this appearance of the blood, and desired I would endeavour to determine, by experiment, whether the ligature's being a longer or shorter time upon the arm, even in the

† Dr. Fothergill.

† Dr. Simson's observation is, that if a tight ligature be made on the limb, and the vein opened three hours after, a size will be produced.—*Vide De Re Medica, Differt. iii. Sect. 38. p. 112.*

ordinary

ordinary way of bleeding, might not influence this appearance of the blood. And accordingly in the presence of Dr. Drummond, Mr. Field, Mr. Hendy, and Mr. Cockson, I made the following experiment.

EXPERIMENT XXXII.

ON the 9th of October, I tied up both the arms of a healthy young man with a degree of tightness sufficient to make the veins swell and become turgid, whilst the pulse remained free; a vein in one arm was opened immediately after making the ligature, and an ounce of blood was received into a cup. I chose to take away so small a quantity that there might be the less probability of producing any change upon the blood by weakening the body. The ligature was left upon the other arm for an hour, which made the veins very turgid, and likewise made the person complain of a
stiffness

stiffness in his fore-arm; the artery in his wrist being felt all the time, but less distinctly than in the other arm which was without a ligature. At the end of an hour this vein was opened, the orifice was large, and an ounce of blood was taken away. Upon attending carefully to each cup, it did appear that the ligature had produced some change; for in the first place, the blood which had been so long detained in the arm by the ligature was darker coloured, or blackish, whilst that from the other arm was more florid, even at its first running from the vein. 2dly, The blood that had been so long in the arm was rather later in being coagulated; for it did not begin to part with its *serum* till at the end of thirty-seven minutes after puncturing the vein; whilst in the other the coagulation was completed, and the *serum* was beginning to ooze out in thirty minutes. 3dly, The blood which was first taken away was without a size, whilst that which had been so long in the other arm had a small spot

spot about the breadth of a silver penny, and did not cover a twentieth part of the surface.

FROM this experiment, therefore, it would seem, that a ligature long continued may produce a size, agreeably to Dr. Simson's observation, but then it will probably be only in small quantity.

THE same learned gentleman, on being informed of the result of this experiment, ingeniously suggested, that the quantity of blood which I had taken away might perhaps be too little to make the experiment decisive; for, as only one ounce of blood had been taken from each arm, all that blood might be supposed to have been contained in the veins themselves; and as it was more probable that the disposition to size took place in the arteries, a larger quantity should be taken away, in order to judge whether the blood in the arteries had not been changed by the veins of the arm being

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so long compressed. The experiment was, therefore, repeated upon the same person, on the 7th of March, Mr. Field and Mr. Hendy assisting me as before, and we observed as follows :

EXPERIMENT XXXIII.

THE blood from the arm first opened was in quantity about ten drachms, had no size, but was late in being completely coagulated. The pellicle first appeared on its surface six minutes after opening the vein, and at the end of fifteen minutes a considerable quantity of the blood was still fluid ; but in thirty-four minutes it was completely coagulated. The *serum* did not begin to ooze out till at the end of fifty minutes.

AFTER the other arm had been tied up an hour, the vein was opened, and about ten drachms of blood were received into the first cup ; as much more into a second ;

second; and about an ounce and an half upon a pewter plate; and about two ounces and an half into a third cup (in all six ounces and an half) and it was observed, that the blood in the third cup and that in the second coagulated in about twenty minutes, and the *serum* began to ooze out in twenty-five minutes after opening the vein; the blood on the plate was later in coagulating, and none of these portions of blood had the least appearance of size. But the blood in the first cup was considerably the latest in jellying; for at the end of twenty-five minutes a large quantity was still fluid under the pellicle, and even at the end of fifty minutes, the coagulation was incomplete. This blood seemed to have rather more size than that in the former experiment, but it was not collected at one part, but was diffused over the surface, and appeared in spots not bigger than pins heads.

THE result therefore of this experiment was similar to the preceding. The ligature long continued produced a size, but in small quantity; and therefore it does not appear probable, that, when the ligature has been only a few minutes on the arm (as is the case in the ordinary manner of bleeding) it can deserve to be taken into the account, when we judge of the disease, or of the indications of cure from the appearance of the size: especially when it is considered, that the blood on which these experiments were made was very favourably circumstanced for Dr. Simson's opinion; that is, it was from a patient who seemed to be plethoric, by the blood which was first taken away not jellying in less than thirty, and thirty-four minutes, which is later than ordinary, as appears from what is observed above, page the 40th, where we found, that the blood of people in health commonly jellies in seven or eight minutes,

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As many of these experiments shew how easily the disposition of the lymph to coagulate can be altered, even by slight changes, as it would seem, in the state of the blood-vessels, they help us to explain how it should happen, that the blood, in some diseases, is found without this property of jellying; an instance of which is mentioned by Mons. de Senac *; another was observed by the learned professor Cullen; and a third I saw lately by the favour of Dr. Huxk and the physicians of the British lying-in hospital, who, having bled a woman in a fever that came on soon after delivery, found her blood did not coagulate on being exposed to the air, but appeared like a mixture of the red globules and *serum* only, the globules having subsided to the bottom in the form of a powder. She died three days after, and, upon opening her, we found the blood had coagulated in her vessels after death, and

* *Traité du Cœur*, Tom. 2. p. 129.

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that a tough white *polypus* was formed in each auricle of the heart, one of which I have now by me. I examined the blood taken away before she died, and found, on exposing it properly to heat, that it did not coagulate sooner than *serum* commonly does, nor under 160° ; so that it is probable, that, at the time she was bled, her blood either was without the coagulable lymph, or its properties were changed.

AFTER a blow or contusion, the blood now and then bursts from the vessels into the cellular membrane, sometimes forming an *ecchymosis*, and sometimes a tumor, and it is a question with some, whether such blood coagulates or not; but that it coagulates in most of these cases, is proved by opening such tumors. Yet it has likewise been observed, that now and then these tumors have been attended with a fluctuation, and that, after some time, their contents have been absorbed,

forbed, and it has also been found, that, upon opening some of them, even several weeks after the accident, the blood was fluid. In such cases the blood had probably undergone a change similar to what was observed to take place in some of the preceding experiments, that is, the lymph had been deprived of its property of coagulating, in passing from the blood-vessels into the tumor; a circumstance, which, how remarkable soever it may appear, agrees with what we have above observed of the lymph, whose properties seemed to vary with the state of the blood-vessels.



C H A P. V.

Containing a recapitulation of the principal facts and conclusions that are mentioned in the preceding pages.

THE separation of the blood into *crassamentum* and *serum*, in a given time, appears to be in proportion as the heat in which it stands is nearer to that of the human body. The heat in which the blood is kept should therefore be attended to, when we draw inferences from the proportions of these two parts.

THE florid colour of the surface of the *crassamentum* seems to be owing to the air. The venous blood, in passing through

through the lungs, has a similar change produced upon it, or becomes more florid by the time it gets into the arteries; and this florid colour is again lost in passing from the small arteries into the veins, especially if the person be in health. But sometimes in diseases it does not undergo this change, but comes florid out of the veins.

NEUTRAL salts, on being mixed with the blood out of the body, make it more florid: they likewise, if used in great quantity, prevent its coagulation when exposed to the air, and some of them allow the lymph to be precipitated, or to jelly on being diluted with water. But we are not to conclude from thence, that they would produce the same effects when used as medicines; for then they are only given in small quantities, and may have their properties changed by the digestive organs, before they are mixed with the blood. And as they are most of them cooling and sedative, they
produce

produce nausea and languor of the stomach, and lessen the force of the vascular system. And as the properties of the blood seem to depend on the action of the vessels, these salts may thus, by affecting the vessels, produce changes on the blood very different from what might have been suspected, from observing what takes place when a large quantity of them is mixed with the blood out of the body.

THE blood is not coagulated (I do not mean thickened, for it is indeed thickened) by cold, but on the contrary, has its disposition to coagulate lessened, and even entirely taken off, if the exposition to cold be long continued. When therefore the blood in the basin jellies, it is not the cold that produces this effect, nor is it the want of motion; for although the blood by being at rest will jelly at last, yet it will not do it in the time the coagulation takes place in the basin;

bason; which in the blood of healthy persons is in seven or eight minutes after being received from the vein. The coagulation of the blood in the bason is therefore probably owing to the air.

WHEN the blood is at rest in the body, it will at last coagulate merely for want of motion; but this coagulation takes place with different appearances from that of the blood in a bason, for it begins in ten or fifteen minutes, and is not completed in three or four hours; whilst the blood of the same animal, taken out of the veins, and exposed to air, will begin to jelly in three or four minutes, and will be completely jellied in seven or eight.

THE effect that air has upon the blood is not immediate on its application, but takes place sooner or later, in different circumstances of health; in some cases the blood is coagulated in a few seconds
after

after it has been exposed to the air, in others not in less than an hour and an half, or perhaps more, as appears from Experiment XIV.

THE inflammatory crust or size is not a new-formed substance, but is merely the coagulable lymph separated from the rest of the blood. This separation seems to be occasioned by the lymph's being attenuated, by which means the red particles soon settle to the bottom, and leave the surface of the blood transparent; and this transparent part being a mixture of the coagulable lymph and the serum, the former coagulates on its surface where, in contact with the air, and the disposition to coagulate being likewise diminished, the blood remains a long time fluid, and thereby gives time for the pellicle formed on its surface to attract the rest of the lymph, and to collect it at the top, leaving the bottom of the cake proportionably softer. The size therefore

fore is thicker, and denser, in proportion as the lymph is thinned, and is less disposed to concrete. But it is not a certain sign of inflammation, and does not appear to be the cause of that disease, but only its effect.

BUT the most remarkable conclusion that these experiments have led to, is, that the properties of the blood depend on the state of the blood-vessels, or that they have a plastic power over it, so as to be able to change its properties in a very short time. The novelty of this opinion, and the difficulty of conceiving how the vessels should have so remarkable a power, has made some object to the conclusion, who had not well considered all the facts from which it was deduced. I shall here therefore sum up these facts.

THAT the blood-vessels, by their stronger or weaker action, can change the properties of the lymph, even in the
short

short time spent in filling the different cups in bleeding, is first inferred from Experiment XIX. where the blood in the first cup had a size, whilst that in the others had none. Now as this want of size in the last cups was owing to the lymph's having by this time become thicker, and to its being more disposed to coagulate, and no other circumstance being observed that could account for this alteration (for the difference in the exposition to cold, or to air, even then, appeared inadequate to the effect, and are now proved to be so by Experiment XXXI.) I concluded, that it was entirely owing to an alteration in the strength with which the vessels acted upon the blood. And to this opinion I was led by the well-known fact, that bleeding weakens the body; and as bleeding, which weakens the body, had here removed the size, by thickening the lymph, and by disposing it more to coagulate, I thence inferred
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that it was by its weakening the body, or the action of the vessels, that it had produced those changes on the lymph. The consideration of what takes place in inflammations confirmed me in this opinion ; for in them the blood-vessels are known to act more strongly, and it is proved above, that the lymph is proportionably thinned, and has its disposition to coagulate proportionably diminished.

AND this opinion was further strengthened, by observing what occurs when an animal is bled to death, or when the vessels are acting with the lowest degree of strength ; for here it was found, that in proportion as the strength of the animal was reduced, the blood was more and more disposed to coagulate.

AND having thus observed the connection between the alterations in the lymph, and changes in the strength of the
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the blood-vessels, in these cases, I next inferred, there might be the same correspondence, even in others where the changes in the properties of the lymph are more sudden, as in Experiment XXVII. where there was no size in the first cup, but a thick one in the third; and even this case, when carefully examined, confirmed the inference; for the blood-vessels were found to be acting with different degrees of strength, at the very time that the lymph was found to have different properties. And the only difficulty that remained here, was to explain how it should happen, that the first cup, contrary to what in general takes place, should have its blood coagulated in less time than the second or third; and this I concluded was owing to some febrile cause, affecting or oppressing the patient; in which conclusion I am confirmed by the fact admitted amongst physicians, that the pulse is frequently oppressed in inflammatory disorders,

disorders, and rises in strength in proportion as blood is taken away.

FROM this conclusion I went further, and conjectured that even temporary exertions of strength in the blood-vessels might alter the properties of the lymph; to which opinion I was led by having observed the blood fizy in the case mentioned in Experiment XX. where great weakness soon followed the evacuation; and likewise from having observed, that the struggles of dying sheep seemed to alter the lymph.

So that upon the whole, the opinion agrees with all the appearances, and is supported by all the differences in strength that occur in the various deviations from the standard of health; for when the vessels act more strongly than they do in health, the lymph is proportionably more thinned, and is less disposed to concrete; and when the ves-

sels act more weakly than in health, then the lymph is proportionably thickened, and is more ready to concrete. Is it not therefore probable, that the differences we observe in the thickness or thinness of the lymph, and in its being more or less disposed to coagulate, are owing to these differences in the strength of the blood-vessels? For such alterations in the strength of the blood-vessels are always connected with those of the lymph, and we can observe no other circumstance connected with those changes of the lymph that can at all explain them.

AND although it must be admitted, that it is very difficult to conceive how the blood-vessels should do this, yet I should hope that ingenious men would not merely on that account reject my conclusion; but would consider, that as it is deduced from a number of experiments, as it agrees with all the appearances, and as it leads to an explanation

planation of many of them which we cannot otherwise account for; it may be well founded although it be difficult to be conceived. For there may be powers in the animal œconomy that are not yet dreamt of in our philosophy.

THIS observation of the properties of the blood depending on the state of the vessels, besides explaining many morbid appearances, leads to a further application; for we may thence be led to advance more rational and more certain rules for the treatment of hemorrhages. For as hemorrhages seem to be stopped, partly by a contraction of the bleeding orifices, and partly by the coagulation of the blood, and as the disposition of the blood to coagulate is increased by weakening the body, and likewise the contraction of the bleeding orifices is promoted by the same means, it is therefore evident, that the medicines to be used, should be such as cool the

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body,

body, and lessen the force of the circulation; and experience teaches us, that such are the most efficacious.

It likewise shews, that all agitation of mind, and all bodily motion should as much as possible be prevented; because they increase the force of the circulation, and are thence unfavourable to the stopping of the hemorrhage. But that languor and faintness being favourable to the coagulation of the blood, and to the contraction of the bleeding orifices, should not be counteracted by stimulating medicines, but on the contrary should be encouraged. And as evacuations weaken the body more when they are sudden, we see a reason why blood-letting should be advisable in hemorrhages, and why a large orifice should be preferable to a small one, when we want to produce that languor or faintness, or that weak action of the vessels, so useful for the stopping of the hemorrhage.

Before

BEFORE we conclude, it may be added, that the practice here proposed is far from being new or uncommon; but as some have recommended opposite methods, and both parties have appealed to experience, where authority so equally balances authority, the young practitioner must be at a loss which to follow, and for want of principles to direct his choice, may frequently adopt the worst practice: witness the use of port-wine, and other stimulating astringents, which is so very common in most parts of England.



C H A P. VI.

Of the Serum of the blood, and particularly of the milk-like Serum.

THE *serum*, when separated from the *crassamentum*, by letting the blood rest in the bason into which it is received, is a fluid, apparently homogeneous and transparent; of a yellowish colour, saltish to the taste, in consistence thicker than water, and its specific gravity, according to Dr. Jurin, is to water as 1030 to 1000*.

* Philos. Transf. No. 361.

WHEN

WHEN chemically examined the *serum* is found to consist of a mucilaginous substance, which is dissolved in a water that contains a small quantity of neutral salts. The mucilaginous substance of the *serum* agrees with the coagulable lymph of the blood in being fixed or coagulated by heat; but the degree necessary for the coagulation of the *serum* is greater than that which is necessary for fixing the lymph, for the lymph is coagulated by a heat between 114 and 120° degrees of Fahrenheit's thermometer; (see Experiment IX.) whilst the *serum* requires 160° to coagulate it: (see Experiment X.)

BUT the degree of heat necessary for their coagulation, is not the only circumstance in which the lymph differs from the mucilage of the *serum*; they differ more remarkably in the former coagulating when exposed to air, whilst the *serum* undergoes no such change.

WHEN the *serum* is coagulated by heat, a watery fluid can be pressed out of it; and this fluid the learned *M. de Senac* distinguishes by the name of Serosity.

THIS serosity contains the neutral salts of the blood, and although it has been exposed to the heat of boiling water, yet it still contains a part of the mucilaginous substance, which is combined with the water in such a manner, as not to be coagulated by the heat of boiling water, till a part of the water is evaporated by boiling; and then it coagulates and appears not very much unlike the mucus spit up from the *aspera arteria* in a morning.

WHEN the mucilaginous part of the *serum* has been coagulated by heat, it cannot again be dissolved in the Serosity, except by putrefaction, or by the addition of some chemical substance, and then
it

it differs from what it was before, particularly in its not being coagulable by heat.

BUT if the *serum* be exposed to a less degree of heat than is required for its coagulation, for example, to that of 100° , it is gradually inspissated into a brownish, solid mass, and this mass can readily be dissolved again merely by the addition of water; and the *serum* seems to possess the same properties that it did before, particularly it is capable of being coagulated by heat. But care must be taken not to add more water than it had lost, for if more be added than was evaporated, it alters its properties of coagulation.

IN this circumstance of being inspissated, and again rendered dissolvable in water, the *serum* agrees with the white of an egg †, but differs from the coagu-

† See Newmann's Chemistry, Sect. IX.

lable

lable lymph, which even when mixed with a neutral salt, (viz. true Glauber's salt) cannot be inspissated, and dissolved again without coagulation.

If fresh *serum* be diluted with an equal quantity of water, and then exposed to heat, it does not coagulate in that of 160° , as when undiluted, nor even in the heat of boiling water, as I have lately observed, but it can now be boiled without immediately coagulating. And as the water evaporates, a pellicle is formed on the surface, which becomes thicker and thicker as the evaporation advances. This pellicle seems to be the mucilage coagulated, for it cannot again be dissolved in water like the inspissated *serum*.

MILK, when boiled, has its mucilage or coagulable part separated in like manner, in the form of a pellicle, in proportion as the evaporation takes place.
And

And both milk and serum, whether diluted or not, agree in being coagulated by rennet *, when exposed to heat. So that milk seems to be made of the mucilaginous part of the *serum*, or is a diluted *serum*, with the addition of an expressed oil, or with a saccharine substance instead of the neutral salts.

BUT although there is an analogy between milk and diluted *serum*, in the circumstance of coagulation, yet they differ in another; namely, diluted *serum* can by a moderate heat be inspissated without coagulating, or forming any pellicle on its surface, but if milk be exposed to the same heat, it is not inspissated so completely; for a pellicle is formed on its surface, in proportion as the evaporation takes place, and this pellicle seems to be as perfectly coagu-

* Which is an infusion of the 4th stomach or the *abomasus* of a calf,

lated

lated as if the milk had been exposed to a boiling heat; for it will not dissolve again merely by adding water, as inspissated *serum* does.

SERUM, therefore, by being diluted, comes near to milk in the circumstance of its coagulation. But the coagulable lymph cannot, by any art yet discovered be made exactly to resemble *serum*.

THE mixture with neutral salts makes it indeed so far approach to the nature of *serum*, as not to be coagulated by exposition to the air; but it does not alter so considerably its property of coagulating by heat, for the mixture with Glauber's salts (in Experiment VII.) coagulated at 125° ; and, I believe, would coagulate in a heat of 123° , if long exposed to it; whilst the pure coagulable lymph is fixed between 114° and $120\frac{1}{2}$, and the *serum* not under 160 degrees of Fahrenheit's thermometer.

Of

Of the white Serum.

ALTHOUGH the *serum* of human blood be naturally transparent, and a little yellowish, yet it is frequently found to have the appearance of whey, and sometimes to have white streaks swimming on its surface like a cream, and now and then to be as white as milk, whilst the *coagulum* is as red as usual. In all these three cases of whiteness, I have examined it in a microscope with a pretty large magnifier, and have found it to contain a number of very small globules, although naturally, when transparent, no globules can be observed in it, notwithstanding what has been affirmed by some authors. These globules differ from the red particles (improperly called globules) in their size, which is much smaller; and likewise in their shape, which is spherical, whilst the red particles are flat. They agree more with the globules of milk.

I have

I have compared them with those of woman's milk, and have found, that in the milk the globules are of different sizes, some being three or four times as large as others, and the smallest little more than just visible, when viewed with a lens of $\frac{1}{3}$ of an inch focus, whilst those of the white *serum* are more regular, and are all of them about the size of the smallest globules of milk. Of this white *serum* I have met with the following instances in books. In Tulpus, one instance *, in Morgagni two †, in the Philosophical Transactions some instances ‡, in Skenckius's Observations two cases are related from other authors §. I have likewise heard of the same appearance having been observed by the learned Sir John Pringle, Dr. Pitcairn,

* Tulp. Ob. l. i. cap. 58.

† Morgagni, Ep. XLIX. Art. 22.

‡ Philosoph. Transact. N^o 100, and 442.

§ Skenckij. Obs. lib. 3.

Dr.

Dr. Hunter, Dr. Watson, Dr. Bromfield, Dr. Garthshore, and Dr. Fothergil of Northampton. And other instances have lately occurred to persons of my acquaintance, who have favoured me with a short account of them.

MR. FRENCH, apothecary in St. Alban-Street, having informed me, that he had some blood by him, taken from a woman the day before, whose serum was as white as milk; he favoured me with a small quantity of it for examination, and with it the following particulars of the case. “ Mary Rider, about twenty-
“ five years of age, of a fresh complexion, and lusty, has not had her
“ menses for these seven months. She
“ discharges blood sometimes by vomiting, and sometimes by stool; complains of a pain in her left side, and
“ in her stomach: she has an inclination
“ to eat, but when she tries, she soon
“ after loathes her food. She complains of great lassitude and sleepiness;
“ her

“ her pulse is ninety-five in a minute.
 “ She has been bled twelve times within
 “ these six months, and every time the
 “ *serum* was as white as milk.”

MR. ROBERTSON, apothecary in Earl-
 Street, acquainted me, that “ Mr. Her-
 “ bert, a publican, of about thirty-five
 “ years of age, and corpulent, had been
 “ subject to a bleeding at the nose, to
 “ the piles, and to such profuse sweats in
 “ the night, as to be frequently obliged
 “ to change his shirt in the morning
 “ before he got out of bed, but that, for
 “ some time past, his sweats had ceased.
 “ That, on September the 23d, he was
 “ seized with a bleeding at his nose,
 “ which had been preceded by a pain in
 “ his head for two or three days. That
 “ his bleeding continued till he had lost
 “ about two pounds of blood, and then
 “ stopt; and that the serum of his blood
 “ was as white as milk. That at ten
 “ o’clock the same night, the hemorrhage
 “ returned, and he lost a considerable
 “ quantity ;

“ quantity; nevertheless, it was thought
“ proper to take sixteen ounces of blood
“ from his arm, during which evacua-
“ tion he fainted, but his bleeding at the
“ nose stopt. That the *serum* of this last
“ blood was likewise very white. That
“ on the 25th, in the morning, he again
“ complained of a pain in his head, and
“ about ten o'clock his nose began to
“ bleed again; but the *serum* now ap-
“ peared no whiter than whey. That
“ he continued to lose blood during most
“ part of the night, so that it was sup-
“ posed he could not lose less than two
“ or three pounds, the *serum* all this
“ time being a little whitish, but so little,
“ that the bottom of the vessel in which
“ it stood could now be seen through it.
“ That his bleeding returned repeatedly
“ till the third of October, when it
“ entirely stopt, the *serum* having be-
“ come more transparent towards the
“ last.”

MR. EUSTACE, apothecary in Jer-
 myn-Street, sent me a phial of white
serum from one of his patients, by trade
 a butcher. "This man," he told me,
 "was tall, of a strong make, a hard
 "drinker, subject to puke every morn-
 "ing, took little food, sweated a good
 "deal, but did not waste in his flesh.
 "He was bled for a slight *asthma* to
 "which he was subject, and of which
 "he had always been relieved by bleed-
 "ing. In other respects he was in a
 "good state of health, so as to follow
 "his business without much inconve-
 "nience."

BESIDES these cases, my friend Mr.
 Lambert, surgeon at Newcastle upon
 Tyne, told me, "that he had a patient
 "some years ago with a violent rheu-
 "matic pain in his hip, whom he was
 "obliged to bleed thrice, and every time
 "his *serum* was as white as milk, but
 "the *coagulum* of its natural colour.
 "This

“This gentleman,” Mr. Lambert adds,
“was a free liver, of a full make, but
“rather muscular than corpulent, and
“remarkable for being a great walker.”

WHEN I first saw this unusual colour
of the *serum*, I was inclined to adopt
the opinion of those who have attempted
to explain it by the patient's being bled
soon after a meal, or before the chyle
was converted into blood. But after-
wards, on considering the cases above
related, I found this could by no means
be the cause, as none of these patients
had taken a sufficient quantity of food to
occasion this appearance; on the con-
trary, most of them had a bad appetite,
and had taken remarkably little food,
and were subject to vomitings. I there-
fore concluded it was owing to something
else, and what confirmed me in this
opinion, was an observation I had re-
peatedly made in dissecting geese, whose
serum I had frequently seen white, whilst

their chyle was transparent; although they had been killed only three or four hours after eating. And as the whiteness, in all the cases that I examined, was owing to a quantity of small globules like those of milk (which are known to be oily) I concluded that these in the human *serum*, when white, were oily likewise, and recollecting to have read somewhere of an experiment by which butter had been got from such human *serum*, I tried, by agitating some of it a little diluted, to separate its oil, or to churn it, but without success. I then inspissated some of it to dryness, and compared it with the natural *serum* of human blood prepared in the same way, and found it less tenacious, and much more inflammable; and when thus dried, its oil oozed out so much as to make the paper in which it was kept greasy. Another portion of this white *serum* being kept some days, putrefied, and when putrid, it jellied as milk does when become sour; but it differed

differed from milk, in being extremely foetid.

Now, as the white globules appear from these experiments to be of an oily nature, and as it is improbable, from these patients having taken little food, and from the transparency of the chyle in birds, that this whiteness of the *serum* should be owing to unassimilated chyle, accumulated in the blood-vessels; we must therefore believe it to be owing to some other cause. And as we know there is a considerable quantity of oil laid up in the cellular substance of animals, which is occasionally re-absorbed, is it not most probable that this curious appearance was, in the abovementioned cases, owing to such a re-absorption? And as all these patients had symptoms of a *plethora*, and were relieved either by spontaneous hemorrhages, or by blood-letting, is it not probable, that, to whatever purpose the oil is applied in the body after it is re-

absorbed from the cellular membrane, in these patients it had been re-absorbed faster than it was applied, and by that means was accumulated in their blood-vessels? This conjecture seems to be confirmed, from considering that in most of these cases the people were inclined to corpulency, and that two of them laboured under a stoppage of a natural evacuation*.

ANOTHER conclusion which these observations lead us to, is, that since the chyle of the birds which I dissected was not white, but transparent, at whatever time after eating it was examined,

* Although it appears probable that the whiteness of the *serum* in the above-mentioned cases was not owing to the chyle, yet I would not conclude that the chyle does not in the human subject occasionally colour the *serum*. We frequently observe the *serum* of such people as are bled a few hours after a meal, a little turbid, like whey, which I believe may be owing to the chyle. But if the milk-like serum was occasioned by a full meal, it is likely we should oftener see it than we do.

it

it follows, that the fat (in these animals at least) is not merely the oily part of the chyle or of the food ; but is a new substance, or a new combination of the principles or elements, which is made probably in the secretory organs of the adipose membrane : the form of oil being made use of by Nature in preference to any other for the nutritious substance of the body, from its being the least liable to putrefaction, and from its containing the greatest quantity of nourishment in the least bulk. This circumstance was clearly proved by my valuable and ingenious friend the late Dr. Stark, who, in a course of curious experiments, made by weighing himself after living for some time on different sorts of food, discovered that a less quantity of suet was sufficient to make up for the waste of his body, than of any other sort of ordinary food ; and that, when compared with the lean part of meat, its nutritive power was, at least, as three to one.

I MAY here add another circumstance that occurred to me when I first thought on this subject, which is, that since we believe the oil, or animal fat, is re-absorbed from the adipose membrane to serve for nourishment to the body; and as some of the patients (whose cases have been related above) could not take food, the re-absorption therefore of this oil might not be so much the cause, as the effects of the disorder under which they then laboured: or, in other words, that upon some defect in the digestive organs, the powers of nature drew from their magazines of oil in the adipose membrane, a supply of that fluid then perhaps necessary for the use of the body. In order to clear up this point, I thought it would be a satisfactory experiment, to compare the *serum* of the blood of animals at different periods after feeding them. For, if the re-absorption of the oil was merely to make up for the want of other food, or, if the *serum* was white merely

merely from a greater quantity of oil being taken up, in order to supply the wants of the body, then the *serum* ought to be whitest in the animal kept longest without food, or whose body was most in want. And as I had found that geese had very commonly this white *serum*, though their chyle was transparent, I chose to make the experiment on them. I therefore took two of them that were very hungry, and feeding both of them with oats, one I killed four hours after, when I knew a part of the oats were undigested; and upon examining the blood, I found the *serum* whitish, and full of small globules; on its being suffered to stand a little time, the white part ascended to the surface like a cream. The other was killed forty-eight hours after eating, when its stomach was found empty, and the *serum* of its blood quite transparent, and without any cream rising to the surface, or any appearance of small globules, when examined with the microscope. Now, this experiment
seemed

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seemed to me decisive, and to point out clearly, that the whiteness of the *serum* was not occasioned merely by the body being in want of food, and therefore, drawing the oil from its magazines; because here the animal most in want of food had its *serum* least white; but was occasioned by the fat's being re-absorbed faster than it was used (from its place being supplied by the fresh chyle) and thence was accumulated in the blood-vessels, so as to give whiteness to the *serum*. And from the same observation it likewise appears probable, that the great re-absorption, and the accumulation of the fat in the vessels of the plethoric patients above-mentioned, was the cause of their want of appetite, and of their other complaints, and not the effect of them.

MAY not therefore a too great re-absorption of the fat, and its accumulation in the blood-vessels, be now admitted as the cause of one species of a *pletbora*?

AND

AND may it not likewise be useful in some complaints of the stomach, to attend to the whiteness of the *serum*? For, although fat be a substance little liable to disease, yet it may perhaps be sometimes so vitiated or may so incommode nature, that she may be obliged to take it up from her magazines, and to use it, or to throw it out of the body. Whilst this is doing, a sickness of the stomach, and want of appetite, may be indications of fulness; and therefore, instead of wanting remedies to strengthen the stomach, may require bleeding, and other evacuations.

APPENDIX,

And may it not likewise be useful in some complaints of the stomach, to attend to the whitens of the Arteries? For although it be a substance little liable to digest, yet it may perhaps be sometimes to vivify or may to incommode nature: that she may be obliged to take it up from her magazines, and to use it, or to throw it out of the body. Whilst this is doing, a sickness of the stomach, and want of appetite, may be indications of tubercles; and therefore, instead of warming remedies to strengthen the stomach, may require bleeding, and other evacuations.



A P P E N D I X,
RELATING TO
T H E D I S C O V E R Y
OF THE
L Y M P H A T I C S Y S T E M
IN BIRDS, FISH, AND THE ANIMALS
CALLED AMPHIBIOUS.

BEING
A Vindication of the AUTHOR's Right
to these Discoveries, in Opposition to
the Claim of Dr. ALEXANDER
MONRO, Professor of Anatomy in
the University of Edinburgh.



A P P E N D I X

RELATING TO

THE DISCOVERY

OF THE

LYMPHATIC SYSTEM

IN FISH, AND THE ANIMALS
CALLED AMPHIBIOUS

BEING

A VINDICATION OF THE AUTHOR'S RIGHT
TO THE DISCOVERY, IN OPPOSITION TO
THE CLAIM OF DR. ALEXANDER
MONRO, FELLOW OF ACADEMY IN
THE UNIVERSITY OF EDINBURGH.



APPENDIX.

AN account of the Lymphatic System in Birds, Fish, and Turtle, was given to the public in the Philosophical Transactions, vol. lviii. and lix. for which communications the Royal Society has since honoured me with their gold medal. These discoveries Professor Monro claimed, in a letter read before that most respectable body on the 19th of January 1769; and has since persisted in that claim, in a pamphlet called, *A State of Facts, &c.* printed at Edinburgh 1770. Now, as both that letter and the pamphlet must of course have been seen by many who know not what can be urged against them, I think it but a duty I owe my own character, to lay before
the

the Public the proofs I have collected of their insufficiency to procure Professor Monro the credit of having anticipated me in those discoveries; and, I hope, that, although in doing this I shall trespass on the time and patience of the reader, yet it will be some excuse for me, that I had endeavoured, as much as could be expected on my part, to settle the dispute without troubling the Public with it.

As Professor Monro has, in this pamphlet, not only endeavoured to vindicate his claim to these discoveries, but has likewise censured me on account of a paper on the *emphysema*, it is necessary, before I come to the controversy about the lymphatics, that I should relate what has passed between us on that occasion.

In the third volume of the Medical Observations and Inquiries, is published a paper on the *emphysema*, in which I proposed the operation of the *paracentesis* of
of

of the thorax, to let air out of the cavity of the chest; which air I endeavoured to shew was the cause of the worst symptoms attending that disease. Not long after this, I was informed that Dr. Monro had declared publicly, he had mentioned that observation in his lectures, both at the time, and before I attended them, which was in the winter 1761, and complained, that I had omitted doing him justice in this particular.

WHEN I heard this, I made inquiries of some of his pupils, who I found had taken notes at his lectures, and by two of these gentlemen I was favoured with excerpts from their notes, which convinced me that he had anticipated me in proposing that improvement. I then determined to let him know, that my omitting to mention his name on that occasion was entirely owing to my ignorance of his claim. This I was the more desirous of doing, from having heard

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that he had exclaimed against me with some acrimony, on the supposition that I had got the hint from him, and was conscious of it ; which being far from the truth, I determined to shew him in what manner I had really made the observation, and thereby stop his exclamations. I determined likewise to shew him that I was desirous of giving him the credit of having had the idea before me, and thereby to prevent all dispute about the matter. The following is a copy of the letter which I sent him on that occasion.

S I R,

BEING informed that you have publicly complained of me, “ for having,
“ in a paper printed in the third volume
“ of the *Medical Observations and In-*
“ *quiries*, omitted doing you the justice
“ of mentioning your having proposed
“ the operation there recommended, in
“ the same circumstances, long before ;”
and as I am confident I deserved not to
be complained of on that account, I have
taken

taken this opportunity of stating the manner of my making the observation, and at the same time of letting you know, that since I have learnt that you likewise had made it, I am willing to do you justice. The thought first occurred to me in reading Mr. Cheston's Pathological Inquiries and Observations, in which he gives a case of the *emphysema*: this case is told in such a manner, that I think it is hardly possible any unprejudiced person should read it and not be convinced, as I was, that the cause of the principal symptoms was air in the cavity of the chest. Mr. Cheston himself, in relating that case, came as near making the observation as possible †. From this hint I prosecuted the subject, as is

† I have since been informed by Mr. Cheston, that I had misunderstood his meaning, when I concluded that he explained the worst symptoms of the *emphysema* on different principles from what I have done, and that he meant to attribute them to air in the chest, and therefore, that the observation was sufficiently made out in his paper; which see in his Pathological Inquiries and Observations, p. 7, and 8.

mentioned in that paper; and before I published it, I consulted every author I could easily procure, who I thought was likely to treat of the subject. And I certainly should have done justice to any that I found had anticipated me, and should not have avoided the opportunity of doing you the same justice. But I knew not, at the time of that publication, that you had ever given the least hint on that subject. About the middle of last summer I was told by a gentleman from Edinburgh of your manner of treating me, at which I was not a little surprised, as I was not conscious of having given you the least cause of complaint. But having since learnt, from other gentlemen who attended your lectures before the time of my publishing that paper (and who, at my request, consulted their notes) that you had really mentioned it, I cannot now doubt that you had made the observation before me. At the same time I must assure you, that to suppose I knew it at the time of publishing

publishing that paper was doing me injustice. Your accusation, I presume, is founded on the supposition of my having heard you deliver the observation at your lectures, when I had the pleasure of attending them. But I do assure you, that if I ever heard the least hint on that subject, either from you, or from any other person, I had not any remembrance of it, at the time I wrote that paper. You are not, indeed, the only person who, as I now find, has anticipated me: the author of the Monthly Review for last June * says, he had long had the same idea, and that he mentioned it in his account of Mr. Cheston's book. But of this too I assure you I was ignorant, when I wrote my paper. What must give farther conviction to any unprejudiced person of my ignorance of your having made the observation, is this: I first mentioned it in a paper which I read to a private society, in which were

* See Monthly Review for June 1768, p. 446.

present many gentlemen that had attended your lectures, and yet all these gentlemen expressed themselves pleased with the observation, as new and interesting, and not one of them gave the least intimation of their having ever heard it before. And yet those gentlemen are as likely to remember any observation which tends to the improvement of physic or surgery, as any I know. I shall mention their names, to justify my good opinion of them; Drs. Stark, Parsons *, Saunders †, Pepys ‡, and Ruston §. The observation was likewise mentioned in another society of young gentlemen, and also in a public hospital, where many, who had been your pupils, heard it, and yet no person

* Professor of anatomy at Christ's-Church, Oxford.

† Physician to Guy's hospital.

‡ Physician to the Middlesex hospital.

§ Some of these gentlemen attended Dr. Monro's lectures about the same time with myself, the others since.

ever told me before I published that paper (which was almost a twelvemonth after I had first mentioned the subject), that you or any other person had ever anticipated me. However, this I relate only to shew I was ignorant at that time of your having made the observation. But now I know that you had, I have not the least unwillingness to acknowledge it, and to do you justice in any future publication. At the same time that I justify my own conduct, give me leave to say, that your manner of treating me (if fairly represented) was not so civil as might have been hoped for. When you complained of me, 'tis a pity you had not likewise hinted there was a possibility of my being ignorant of your having had the idea. You might perhaps too without impropriety have hinted, that should it come to my ears that you had anticipated me, I might possibly be capable of such an exertion of candour as to acknowledge it. But, to have done with suppositions, this at least I am sure

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of, that though I may be as covetous of fame as most people, yet I am incapable of taking any unjustifiable methods of acquiring it.

I am, Sir, &c.

Dec. 31, 1768.

THIS letter, Professor Monro could not but acknowledge, “ sufficiently satisfied him in having secured his title, as “ the first who had proposed that improvement.” Yet so unfair an account does he give of it in his State of Facts, that he only says, I acknowledged, that “ I could not doubt he had made the observation before me; but the farther “ particulars of it (he adds) it is needless “ to trouble the reader with, since as “ much as is necessary of these will be “ sufficiently understood, from his letter “ in answer” to me, which, surely! is not the case; for it no where appears in his letter, that, besides mentioning my conviction of his having anticipated me,

me, I had likewise promised to do him justice in a future publication. Nor does it appear in his letter, that I had in mine shewn how little probability there was of my having got the idea from him. These the reader may perhaps think Professor Monroe ought to have declared, in justice to me. For what more could be expected of me, seeing I had by accident hit upon an observation, which, as it happened, he had made before, than to acknowledge the priority of his title, as soon as I knew it, and to put that letter into his hands by which he might always be sure of securing to himself what was his due. But Professor Monroe says, it was unnecessary to give a fuller account of my letter. But why was it so? Not surely in justice to me, nor for the satisfaction of the reader.—Nay, so far is Dr. Monroe from doing me justice on this occasion, that he even intimates I rejected tapping the chest with a *trocar*, because it happened to be his method, as
if

if the same was not the method recommended by many of the writers on the subject of the *paracentesis* of the thorax, for the cases in which they advise that operation, to whose method I alluded, and not to his, which I then knew nothing about.

NEXT, as to the discovery of the Lacteals and Lymphatics in Birds, Fish, and the animals called Amphibious; of these an account was laid before the Royal Society on December the 8th, 1768. I was present when it was read, and had afterwards some conversation on the subject with Dr. Donald Monro, who, as appears by the sequel, informed his brother, the Professor, of what I had done. Not long after this, I again saw Dr. Donald at St. George's Hospital, and he then told me, that the *Lymphatics* and *Lacteals* in those animals * had

* Meaning Birds and Fish.

been

been discovered by his brother eight years ago, as he now learnt by a letter from Edinburgh, a part of which letter he was to shew to every body, and which was already given to be read before the Royal Society. When I was informed of this, I was astonished, as I remembered to have heard the Professor, since that time, declare that they were not discovered. Besides, I had a note taken from his lectures within two years of his making this claim, in which was a similar acknowledgment *. I was convinced therefore that he had no title to these discoveries. Upon which I laid before the Royal Society my reasons for that conviction, in a letter to one of their secretaries. Of this letter I shall give the reader an account, but shall first lay before him a literal copy of Dr. Monro's claim.

* That note is now printed below, p 176.

Copy

*Copy of Dr. Alexander Monro's claim,
&c. read before the Royal Society,
Jan. 19, 1769.*

“ ABOVE four years ago (says he) I
“ injected the lacteal vessels of a turtle,
“ or sea-tortoise, with quicksilver, after
“ injecting the artery and vein with wax,
“ and have shewn this instance of the
“ vessels in the oviparous animals every
“ year in my college, and had a draw-
“ ing made of it two years ago by Dr.
“ Palmer, a copy of which I have sent
“ inclosed, engraved by Donaldson.

“ I LIKEWISE, eight years ago, men-
“ tioned those vessels in Fowls and Fishes,
“ which I had seen, but not injected.”

HERE then is an assertion about the
vessels which I had discovered, that is far
from being equivocal. For here he
affirms,

affirms, that he really had seen them eight years ago, nay, that he had even mentioned them to others. This letter too was sent immediately after he heard that I had laid before the Society an account of those vessels in Birds and Fish *. It could not therefore be meant merely to inform the Society that, now seeing Mr. H. had discovered the Lacteals and Lymphatics in Birds and Fish, he likewise had the pleasure of shewing them that he (Dr. Monro) had discovered them in the turtle. This, I say, could not be his meaning; for if it was, why did he send his letter so precipitately? Why did he not send a description of those vessels in the turtle, in order to make his letter worthy their notice? And why did he say he had seen them in Birds and Fish? The Society, he knew, wanted not his testimony to prove that Birds and Fish had them. What then could he mean by it, but to claim the discovery?

* As he acknowledges, State of Facts, p. 4.

As there could be no doubt that Professor Monro meant this letter as a claim to these discoveries; neither had I any doubt but I should, for the reasons above-mentioned, prove that he had no right to them. In order therefore to prevent the prejudices that might arise against my papers, from his being believed to have anticipated me in these discoveries, I wrote a letter to one of the secretaries of the Royal Society, in which I first shewed, that I had seen the Lacteals of the turtle about a year before him, and then, when I came to speak of those vessels in Birds and Fish, against the probability of Dr. Monro's having anticipated me in these discoveries, I made use of the following arguments.

1st, His not having, by his own confession †, injected them, which he certainly would have done, in order to complete the discovery. To which I

† In his claim; see above, p. 172.

observed

observed he had the strongest motive, both from his knowing the importance of the subject, and from his having unfortunately declared, in the 57th page of his Anatomical and Physiological Observations, printed at Edinburgh, 1758, "that, "after a considerable number of experiments which he had made, he was "convinced, that neither Birds, Fishes, "nor oviparous animals in general had "either Lacteals or Lymphatic Vessels."

After which declaration, I conceived it improbable he should patiently wait eight years without injecting them, especially as I had found it an easy matter to inject them, when once they were discovered. And I added, the probability was, that if he had *seen* those vessels, he would have hastened to inject them, and to complete the discovery, were it only to prevent another person's doing it, and thereby acquiring the reputation of having done what he himself had *in vain* attempted by such a considerable number
of

of experiments as *were sufficient to convince him*, that such vessels existed not in those animals.

2dly, I SAID, his claim to the discovery of those vessels, by affirming he had seen them eight years ago, was contradicted by public declarations made after that time; for he had, since, acknowledged in his lectures, *that he had sought for them in vain by a variety of experiments*. And even so late as within these two years †, he declared likewise in his public lectures, “ that the Lymphatic System was supposed to take place only
“ in men and viviparous animals, and
“ by analogy in those fishes placed by
“ Linnæus amongst the *mammalia*, and
“ how far was their just extent (he said)
“ was not certain, but that he had found
“ them in some amphibious animals, as
“ in the turtle.” These declarations, I

† My letter being dated Jan. 10, 1769.

observed,

observed, were inconsistent with his claim to the discovery.

BESIDES using these arguments, I promised the Society I would hereafter produce unquestionable proofs of the invalidity of his claim, having by this time found, that the Doctor, fortunately for me, had expressly acknowledged in his lectures, *that he had sought for them in vain*, almost every year since the time that he now pretends to have seen them.

DR. MONRO being informed of these proceedings, sent me his letter, dated June the 8th, which he has since printed in his *State of Facts*. But that letter appeared so confused, that I knew not what to make of it. Sometimes I thought it was meant to prove that he had discovered those vessels, agreeably to his assertion read before the Royal Society: but this I soon after suspected could not

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be the case, because, after relating all his facts and experiments, he concludes, *not* that he had discovered them, *but only* “that he had seen what he strongly “*suspected* to be lacteals in those animals” (*viz.* Birds *)—And, “that “(from preceding experiments) he was “*persuaded* that Birds were provided “with lacteal vessels, and confirmed in “this *opinion*, by having injected them “in one of the same oviparous class, “the turtle †.”—Or, in other words, Dr. Monro claimed a discovery, by telling the Royal Society that he had *seen* those vessels in Birds eight years ago, which he now proves, by shewing he had only *suspensions* about them, and an *opinion* that Birds had them, because Turtles had them.

At other times, I thought that possibly, after finding what his pupils testi-

* State of Facts, p. 21.

† Ibid. p. 23.

fied, he might now be convinced he had imprudently claimed those discoveries, and might intend this as a sort of an acknowledgment (tho' an awkward one) of my being the first who had seen those vessels. But the vindictive stile of his letter convinced me it was not meant as an apology †, as likewise did the stile of a short note written on the cover of that letter, of which note the following is a copy.

TO MR. HEWSON, &c.

S I R,

WHEN you have read the inclosed, you are very welcome to write such remarks on it, as to you, or to your friend Dr. H——r, or to any of his friends,

† As for instance, where he talks “ of (his) “ making all the allowance I require for my natural, “ or (says he) I should rather call it, unnatural “ imbecility of memory.” This passage is altered in his *State of Fasts*, p. 22.

such as Dr. —, &c. may seem proper; only when you have done, I think you ought to shew it to all those societies, physicians, and students to whom you have made free with my name; or, if this task should not suit your disposition, or be irksome to you, after the great fatigue you have taken about me already, please to let me know this, and I shall take that trouble on myself.

I am, Sir, &c.

(Signed) ALEX. MONRO.

Edinburgh, June 24,
1769.

THIS seemed clearly not to be the stile of one who was sensible of his error, and was apologizing for it; and convinced me, that Professor Monro intended that letter as a proof of his right to those discoveries. However, not to be positive
that

that I had hit upon his meaning, I determined, before I laid any thing before the public, to ask an explanation of that letter. For this purpose I wrote to him on the 15th of July, and desired him to tell me “whether he meant it as a
“proof of his right to those discoveries.”—or “whether he meant by
“it to give up to me the right to them.” And as I had found him in that letter wandering from the subject, and instead of concluding that he *really* had seen those vessels, concluding *only*, that he had seen what he *suspected* to be the Lacteals in Birds—And again, that he was *persuaded* Birds were provided with those vessels—but no where saying that he had seen what he *knew* or *could prove* to be their Lacteals, which alone could give him a right to the discovery: I therefore told him, “that to avoid for
“the future all wandering from the
“subject, I should state the dispute as
“it appeared to me;” and then I said, that “it was he who began it, for, on

" hearing that I had discovered the
 " Lymphatic System in the three classes
 " of oviparous animals, he had sent a
 " letter, which was read before the
 " Royal Society, and was to be shewn
 " to every body. In this letter he
 " asserted, that he had discovered the
 " Lacteals in a Turtle about four years
 " ago, and in Birds and Fish eight years
 " ago; and that he even mentioned
 " these discoveries to others." These
 assertions (I added) were construed a
 claim to the discoveries I had made.
 With this letter I likewise sent him a
 copy of mine, which had been read be-
 fore the Royal Society. By these means
 I thought I should either keep him to the
 subject in question, or, if he should
 again wander, the reader would be con-
 vinced it was not my fault, but his own,
 that he *now* knew not what he had *then*
 asserted.

THIS letter, however, had no effect.
 I therefore wrote to him again, hoping
 he

He might now be convinced that his claim was ill-founded, and might therefore be induced to retract it, instead of obliging me to prove to the world its invalidity. The following is a copy of the letter which I sent him on that occasion :

S I R,

It is now above six weeks since I wrote to you, desiring an explanation of your letter of the 8th of June. As you have not given me that explanation, I have now taken up the pen to inform you, that agreeably to your own desire, and in order to justify my conduct towards you, I am commenting upon that letter which you sent me. My comment would be more to the purpose, were I always sure I understood you, but if that satisfaction should still be denied me, I must proceed as well as I can, and I must say, that if I should mistake your meaning, it will not be wilfully, since

N 4

you

you might, by an answer, have cleared up all ambiguity. I cannot help regretting, that this dispute should subsist between us, both on my own account, as I think it hard to have the trouble of proving my right to discoveries which are certainly my own, particularly as it takes up that time which I hoped to employ to a better purpose; and I likewise regret it on yours, since, in order to maintain my right, I shall be under the necessity of producing some facts and testimonies, which, in my opinion, cannot but lead to conclusions very unfavourable to your reputation. And as I should be sorry that one of my first attempts to lay the foundation of my own character, should be attended with circumstances which may hurt yours, and really wish to avoid it; I therefore still hope that this dispute may be settled in a more easy manner. You must, I think be now convinced, that in claiming these discoveries you have injured me, and cannot be at a loss to know what might be

be expected from you on such an occasion. But if instead of doing me that justice which might be expected from a man of candour, you treat this letter likewise with silence, then justice to myself requires that I should no longer delay producing such proofs as I possess of your having no right to these discoveries, and shewing them to the very respectable Society, to which I have promised them; or to such *physicians, students, &c.* as may have heard of your claim; without regretting much that those measures which I take to maintain my right, may perhaps affect sensibly the character of a man, who having first injured me and afterwards had his error pointed out to him, was incapable of candidly acknowledging it.

I am, Sir, &c.

(Dated)

Sept. 9, 1769.

IN

IN answer to this letter, he sent me one dated Sept. 30, in which, instead of answering my questions, he evades them, concludes as vaguely as in his former, and when he speaks of his assertion read before the Royal Society, alters its sense, qualifying the alteration with "*to the best of his recollection*" denies he was mistaken in claiming these discoveries. And, what is still more remarkable, accuses me of having made conclusions injurious to him, "*by arguments, weak, inconclusive, not real, but feigned.*"

It was evident, from such a letter, that he would not embrace the opportunity I offered him, and avoid a dispute, by acknowledging his mistake, and retracting his claim. I therefore no longer hesitated to print the proofs I had collected of his not having anticipated me; and though I had once intended to make some remarks on his letter of the 8th of June, as is mentioned above, yet I afterwards determined

determined to omit these, since the testimonies of his pupils alone sufficiently proved that he had no right to those discoveries. By these means I reduced the publication to half a sheet of paper: in which I first gave an account of his claim made, by saying "*he had seen those vessels* EIGHT years ago;" then I mentioned, as arguments against its *validity*, that I had myself heard him since that time declare, "*he could not find those vessels,*" and that, besides, I had a note taken at his lectures by a gentleman within two years of his making it, which contained a similar declaration *, and afterwards

* Dr. Monro has misinterpreted this passage. He supposes I meant that this note was taken two years before 1762, whereas I meant two years before 1768, the time when he claimed these discoveries, and when I wrote my letter to the secretary of the Royal Society. The note itself is printed above, page 176.

Dr. Monro has taken notice of another inaccuracy, that is, where I had said, "He asserted that
" he

afterwards I said that I had written to such gentlemen as I knew had, as well as myself, attended his lectures within these eight years, desiring them to consult their notes, and to let me know what Dr. Monro had said as to the existence, or non-existence, of the lacteals and lymphatics in these animals; without mentioning the dispute between us, or any opinion I had formed, that they might be unprejudiced to either party. And that such of these gentlemen as had taken notes sent me excerpts from them, which, as I had suspected, agreed with what I had myself heard the Professor say upon that subject.

THAT Dr. Haygarth, physician at Chester, had sent me the following pas-

“ he had anticipated me in these discoveries ;” instead of saying “ that he claimed them by asserting “ he had injected, &c.” The former Dr. Monro seems to allow to be what he meant, but not *exactly* what he said. It was therefore a small inaccuracy. But his claim is now printed *verbatim*. See above, p. 172.

fage

page from the notes which he had taken in 1764.

“ FOWLS, and some Fish (says Dr. Monro) have not lacteal vessels that we can see; they have no conglobate glands in the mesentery; perhaps they (the lacteals) don't run into each other, but into red veins, and hence never are so large as to become visible.”

“ THIS note,” adds Dr. Haygarth, “ was taken in 1764, and if Dr. Monro had changed his sentiments on this subject in the year 1765, I should certainly have taken notice of so remarkable a circumstance.”

THAT, from Mr. Orred, surgeon at Chester, who attended Dr. Monro's lectures during the winter 1765-6, I had learnt, that Dr. Monro, when he spoke of the anatomy of a cock, declared: “ he

“ never

" never saw, or observed any *glandulæ*
 " *vagæ*, or lacteals, but had seen lym-
 " phatics in the neck, ending in the
 " jugular*."

THAT, from some notes, said to be copied from those taken by Dr. Taylor of Reading, in the winter 1765-6, I had procured the following excerpt; and Dr. Taylor, on being requested to consult his own copy, had acknowledged it was a just one.

" THE lymphatic system (says Dr. Monro) is said to take place only in
 " men, and viviparous animals, and
 " from analogy in those fishes placed by
 " Linnæus, under the class of Mam-

* It may be necessary to mention here, that the dispute between Dr. Monro and me is, *who first discovered the lacteals of birds?* for as to the *lymphatics* in their necks, (mentioned in this gentleman's note,) these we both allow, were discovered by Mr. John Hunter about ten years ago.

" *malia* :

APPENDIX. 191

“ *malia* *: how far is their just extent
 “ is not certain, but we have found
 “ them in some amphibious animals, as
 “ in the turtle †.

“ It is said that this system is want-
 “ ing in oviparous animals; but this is
 “ not universally true; for we men-
 “ tioned, that we found them in a
 “ turtle, and they would probably be
 “ found in other orders and genera, if
 “ properly examined. But admitting
 “ that they are not demonstrable there,

* In the excerpt it is *amphibia*; but it is evident
 from the sense, and from comparing it with the
 other notes, that it should be *mammalia*.

† As to the *lactals* of the turtle, there is no
 doubt but that Professor Menzies and myself have
 both discovered them. He, in the summer 1765;
 I before that time, viz. in the autumn 1763; when
 I took a short description of those vessels, which is
 published with my paper on the lymphatic system
 in birds, in the 58th volume of the Philosophical
 Transactions.

“ it

“ it doth not follow that they are want-
 “ ing ; for, perhaps, they may run
 “ only a little way, and terminate in red
 “ veins.”

THAT Dr. Maddocks, phyfician to the
 London Hofpital, had favoured me with
 the following excerpt from notes which
 he took at Dr. Monro's lectures, in the
 winter 1765-6.

“ LYMPHATICS are found in vivi-
 “ parous animals, and therefore, I pre-
 “ fume, in the whale, which is of this
 “ kind. They are not to be found in
 “ oviparous ones, fishes, nor the *am-*
 “ *phibia*: this is the common doctrine.
 “ *I will not say how far they may be*
 “ *found in some birds*, but I have found
 “ them in some of the amphibious ani-
 “ mals, as in the turtle, running along
 “ the root of the mefentery.”

THAT

THAT Mr. Hull, surgeon at Steven-
age, had sent me the following excerpt
from his notes, taken about four years
ago.

" I NEVER could, to this day, (says
" Dr. Monro) find a single branch of a
" lacteal in the abdomen of fowls, nor
" any lacteals, or glands of the conglo-
" bate kind in the mesentery, notwith-
" standing I have made experiments
" with that view very often. I kept
" fowls twenty-four hours without food,
" then fed them with bread soaked in
" milk, and tinged it by turns with
" blue, madder, and saffron, and after-
" wards opened them at several different
" times, in order to discover the lacteals,
" but all without success. Yet, per-
" haps the lacteals may be in fowls,
" though not demonstrable." This,
adds Mr. Hull, I will answer for being
verbatim, or nearly so, as Dr. Monro
delivered it in the anatomical hall, at

O

Edin-

Edinburgh, on the 13th of February,
1765*.

THESE passages, I added, were sufficient to shew how little right Dr. Monro had to these discoveries. Besides, I said it was a strong argument against him, that in the letter I had received (which he has printed in his State of Facts, p. 8.) he could not, after relating all his experiments and observations, conclude he had *really* seen those vessels as he had told the Royal Society; but in one place he says, “ that, from the preceding
“ experiments, &c. it is evident that he
“ had seen what he *suspected* to be the
“ lacteals in birds.” And in another,
“ that he was himself *persuaded* that

* If the reader will take the trouble of comparing this note with Dr. Monro's own account of his experiments, (State of Facts, p. 12.) he will be convinced how accurately this gentleman must have taken notes.

“ birds

“ birds were provided with lacteal vessels, and confirmed in this *opinion*, by having now injected them in one of the same oviparous class, the turtle †.”

SUCH conclusions appear merely evasive, and never can be considered as proofs of his having discovered those vessels before I did, agreeably to his assertion read before the Royal Society, and since repeated in his State of Facts.

THE half-sheet of paper, containing these arguments and testimonies against Dr. Monro, was printed Dec. 1. 1769, and was given to such gentlemen of my acquaintance as had heard of his claim, and a copy of it was sent to the Doctor. Upon receiving which, he published his State of Facts; but what is singular, he

† This argument was repeated in a note, to prevent Dr. Monro's writing; as if the dispute between us was, *who first had suspicions about those vessels*, instead of *who first discovered them*.

has attempted his justification without taking proper notice of these testimonies against him; as if he could be justified whilst they remain unanswered. And in this State of Facts, in spite of those testimonies, he repeats to us, "that long before 1762, he observed blueish vessels in the mesentery (of birds) which he judged to be lacteals, and had mentioned as such in his lectures *." And again, "that about the years 1759-60, he had seen collapsed blueish vessels, which he concluded lacteals, &c." †. What shall we say of this?

NAY, Dr. Monro has, upon this occasion, even ventured another assertion, viz. "that the notes of his pupils taken for three years before 1762, will be found to prove, that he then taught the direct contrary" of what I have brought these testimonies to prove he has

* State of Facts, p. 4.

† Ibid. p. 27.

since

since taught †. Now, surely this is very improbable; and Dr. Monro should have adduced some testimonies to prove it. But supposing it were true, it would lead to a conclusion unfavourable to him. It would shew, that he must have misled either the one set of gentlemen or the other;—for he says, he told the first he had seen the lacteals—the last prove he has since taught them that he *never could see* those vessels.

THE reader, I fancy, by this time thinks with me, that Professor Monro's claim deserves no more of our attention. But, as he has printed some excerpts from his own book of notes, with the parade of having them authenticated, as if they contained the discovery, notwithstanding the above-mentioned proofs of his having acknowledged repeatedly since he wrote them, that he never could find

† State of Facts, p. 26, in the note.

those vessels, I shall next, therefore, make some remarks upon his notes.

To begin with those relating to the turtle. He discovered its lacteals in the summer 1765. I had seen them before that time, viz. in the autumn 1763. Besides, I have since injected and traced out the whole system*; he does not even pretend to have done so: it is, therefore, not difficult to determine, who was the first discoverer, and who has carried the discovery farthest.

NEXT, as to the lacteals in Fish. To prove that he had found those vessels eight years ago, he tells us, that in a note taken from the dissection of a skate on April 24, 1760, he has said, “ He
“ had discovered a whole system of
“ lacteals and lymphatic vessels running
“ towards the heart, on the left of, and

* See Philosoph. Transact. vol. lix.

“ above

“ above the *vena portarum*, and from
 “ these the auricle of the heart was
 “ blown up. They are proportionally
 “ larger, but have fewer valves than in
 “ man †” Now, I will take upon me
 to say, there is nothing in this note
 which proves whether he had inflated a
 lacteal or a vein. For what he says of
 the situation of the vessels, and of his
 blowing up the heart, is equivocal: the
 only part of the note which appears to
 characterise the lacteals, is in reality a
 mistake; that is, where he says *they have*
valves. But the lacteals on the mesentery
 of a skate have no valves, and injections
 pass readily from the large to the smaller
 branches. And what is even more to
 the purpose, although it appears, from
 his calling what he saw *lacteals* and *lym-*
phatics, that he had at that time some
 suspicions about them. Yet I am per-
 suaded he has since changed his opinion;
 and this I think is evident, even from the

† See State of Facts, p. 12.

manner in which he speaks of his experiments made the year after. For, says he, "I have dissected this year (1761, "in summer) eight skates, and about a "like number of cods and codlings, but "without being able to observe by dis- "section, or to inflate any like to lacteal "or lymphatic *glands*—I find indeed " (he adds), that blowing backwards in "the meseraic veins, the intestines and "the cellular substance between their "coats are inflated; but this is no direct "proof of branches of red veins ab- "sorbing, as these veins may be burst, "or the air may have first entered the "arteries *." Now, this surely is not the language of a man who had seen the lacteals, but of one that was seeking for them. Had he found them, he certainly would have mentioned it in this note, but he avoids the subject entirely, and only says he could not find the *glands*, thereby leaving us to suppose that these

* State of Facts, p. 12.

dissections were made for the *glands only*, after having discovered the *vessels*: which is highly improbable, since, by his own confession †, he did not inject the vessels, which he knows well enough is the best way of determining whether the glands exist or not; and *one* experiment in this way would have been more satisfactory than his *eight*, or than eight hundred made by dissection only ‡. Add
to

† See his claim above, p. 172.

‡ If the reader should happen not to be well-acquainted with this part of anatomy, he may not see all the force of this argument, which will be satisfactory to anatomists; for it is a fact admitted amongst them, that the mesenteric glands are placed only in the course of the lacteals, so that the lacteals must pass through these glands in their way to the heart. The readiest method therefore of discovering the glands, after having seen the vessels, is by injecting those vessels; for the injection, in its way to the heart, must distend the glands, and make even the smallest of them visible. The vessels seen by the Doctor seem to have been very large; can it be supposed then, if he had been convinced they were lacteals, that he would not have injected them,

to this, he would not, I think, if he had now seen the lacteals, have taken up his time with trying whether the red veins did the office of absorption for them, as he seems to have done by blowing into these veins. Nay, I will go farther, and will take upon me to say, that it is probable he was in these last dissections convinced he had been mistaken in what he took the year before for lacteals and lymphatics. This I think evident, both from the notes above-mentioned, and from his manner of treating the subject since that time. For, if he thought he had seen those vessels, he would doubtless have used this discovery as an argument against absorption by the common veins, as he has since used that in the turtle. But it appears, from the notes of his pupils †, and even

them, and thus have determined whether there were glands or not, by one experiment, instead of tediously dissecting sixteen Fish?

† See his pupils notes above, p. 189, &c.

from

from his own account of those arguments †, that he has not done so. And again, had he thought he had discovered those vessels, he would not have acknowledged in his lectures since that time, that they were not yet known to exist ‡. He has therefore aggravated the impropriety of his conduct in claiming these discoveries, by the disingenuity of sending such notes as proofs of his claim.

AND lastly, as to the lacteals in *birds*, he tells us, “ that, in 1758, he re-
 “ marked a vessel making an arch on
 “ the mesentery of a cock, which at
 “ first he believed to be a trunk receiving
 “ the lacteals, but not being able to in-
 “ ject it on trial, he conjectured to be
 “ rather a nerve.” And afterwards,
 in April 1759, “ he observed in a cock

† See State of Facts, p. 16.

‡ See the notes of his pupils above, p. 189.

“ what

“ what *looked like* lacteal vessels col-
“ lapsed, of a blueish colour, which
“ seemed to terminate at the back-bone,
“ &c.—These he shewed to the stu-
“ dents.” And again, after relating
the manner of making his experiments
upon no less than twelve cocks*, he
tells us, “ that in 1761 he observed, in
“ the interstices of the great arches of the
“ red mesenteric vessels, a pellucid net-
“ work, some part of which seems to be
“ composed of branches sent from a
“ large nerve, running parallel with the
“ intestines, and nearer to them than
“ where the trunk of the mesenteric
“ artery sends off its large branches;
“ but although (says he) *I suspect*
“ *strongly* there are here too numerous
“ lacteals, and I even observe very small
“ knots, which I take to be analogous

* The experiments were made by feeding these birds with oatmeal and madder, oatmeal and rhubarb, &c. See State of Facts, p. 12.

“ to our mesenteric glands, yet I have
 “ not observed the above-mentioned
 “ kinds of food to make any odds in their
 “ appearance *, &c.” And again, after
 a variety of other experiments, he says,
 “ he could not observe more than above-
 “ described.” Now, what is there in
 these notes that can entitle him to the
 discovery of the lacteals in birds?—Can
 his seeing a blue plexus on the mesentery,
 which at first indeed he suspected to be
 lymphatic, but afterwards to be nervous,
 and a part of which, he acknowledges,
 was in his last experiments found to be
 made of nerves, entitle him to it? or
 can his discovery of these small knots,
 which he takes to be analogous to our
 glands, entitle him to it? Certainly not.
 Birds have no lymphatic glands on their
 mesenteries, as I have shewn †. Is it
 not therefore plain, from these notes

* State of Facts, p. 12.

† Philosoph. Transact. vol. lviii. art. 34.

themselves,

themselves, that he had not discovered the lacteals in birds? Has he not repeatedly since that time acknowledged this in his lectures *? What shall we say then to his asserting that he had seen them eight years ago, and his laying before a respectable Society, and desiring his brother to propagate such an assertion? Or what shall we say to his persisting in it, or above all to his telling us, in his last publication (p. 26.) “ that
 “ in these notes the reader will find the
 “ appearance of these vessels after death
 “ really described †?”

AFTER these notes follow some others, to prove that he had *argued* in favour of the probability of the existence of those

* See the notes of his students above, p. 189.

† Let me beg of the reader again to examine these notes, and then judge of the propriety of Dr. Monro's affirming they really contain a description of those vessels, when he has himself put it in the power of the reader to observe the contrary.

vessels in Birds and Fish; and a conclusion that he had *supposed* frogs might have them; and his *suspensions* that Birds might have them—And his *persuasions* that they must have them (*not because he had seen them, but*) because turtles had them. Which are nothing to the purpose, and ought never to have induced him to claim the discovery, or to say he (actually) had seen them. I cannot therefore think it worth while to take any farther notice of these conclusions.

It is indeed remarkable, that Dr. Monro could persuade himself he had any original merit *even* in entertaining such *suspensions* and *opinions*. More than one writer had suspected those animals had them, and that they themselves had seen something like them; for a proof of which the reader need look no farther than Dr. Haller's *Elem. Phys.* *. But, as those writers had given no proofs of

* Lib. ii. Sect. 3. & Lib. xxiv. Sect. 2.

their

their having discovered them, their *sus-*
pitions and *opinions* past for nothing.

PROFESSOR Monro, not satisfied with claiming these discoveries, has even gone farther ; he has intimated, in some parts of his book, that I might have learnt them, or a part of them, from him. As in page 4, where he speaks of my “ giving an account of these vessels entirely as my own discovery,” this in page 6. he calls “ broaching another “ subject with him ;” and complains of me “ for passing in silence what I might “ have heard him observe concerning it “ when I attended his lectures.”—How Professor Monro could pretend that I had learnt any thing from him on this subject, that ought not *for his sake* to be passed in silence, is astonishing. What could I learn from one who has repeatedly since that time acknowledged *he never saw* these vessels ; *that they might be too short to become visible* ; and who,

at

at the time I attended his lectures, said, he could not find them, as I have already declared. But, as my testimony will have more weight with the reader, when corroborated with that of a gentleman unconcerned in the dispute, I shall next add a copy of some notes taken by Dr. Morgan, now professor of medicine in Philadelphia, who attended Dr. Monro's lectures at the same time with myself, and who, at my request, sent me the following excerpt, taken at his lecture upon the question, Whether the common veins absorb or not?

“ Most authors (says Dr. Monro)
 “ concurring in opinion, that fowls
 “ were destitute of lymphatics, and not
 “ being able to discover them myself, I
 “ was led to be of their opinion. I have
 “ already observed, that where conglu-
 “ bate glands are found, there are
 “ lymphatics, and the converse of this
 P “ pro-

“ proposition, namely, where there is
“ no conglobate gland, there are no
“ lymphatics. And there being no con-
“ globate glands to be seen in the
“ mesentery of fowls, nor in fishes, I
“ judged these animals to be destitute of
“ lymphatics; but Mr. John Hunter
“ having discovered conglobate glands
“ in the neck of a swan, put me on fur-
“ ther search, and I then found them
“ plainly in common fowls, *but never*
“ *could find any lacteals* in their mesen-
“ tery, though experiments were tried
“ by means of coloured tinctures of va-
“ rious sorts, as of rhubarb, &c.”

FROM this excerpt it is evident, that
Professor Monro, when I attended his
lectures, taught, as he has since done,
that what he knew of the lymphatics he
learned from Mr. Hunter, and as to the
lacteals *he could not find them*; and this
was in the spring, 1762, the very year
after the time when, according to his
letter

letter read before the Royal Society, he should have seen these vessels, and mentioned them in his lectures : and finally, to complete the whole, he now complains of me for passing in silence what I might then have heard him observe concerning them.

Thus have I endeavoured to obviate the arguments in Dr. Monro's publication, and the reader must now, I think, see clearly, not only the impropriety of the Doctor's asserting his right to these discoveries, but the still greater impropriety of his persisting in that assertion.

BESIDES claiming these discoveries, Dr. Monro has, in his letters on the subject, treated me in a manner which I cannot pass unnoticed.—Thus, he first gives the name of misinterpretation to my concluding from the notes of his

pupils, that he had not seen " what
 " he believed" the lacteals, and then
 adds :

" SHOULD we even suppose the above
 " misinterpretation venial, what must
 " the reader think, when he is told,
 " you was informed that a gentleman,
 " who had attended my lectures two
 " years at least before I injected the
 " lacteals of a turtle, that is, nearly
 " about the time you did, declared he
 " heard me then speak of having *seen*
 " *the lacteals in fowls*; and yet that
 " you continued to vent this injurious
 " supposition? That is, you must have
 " sunk this material information, since
 " it overturned the whole purport of
 " your story *."

Now, here is an accusation, which,
 were it true, would fall heavy upon me.

* See his State of Facts, p. 23.

But

But the case is this; I had indeed heard that a gentleman, who attended Professor Monro's lectures about the time I did, had declared he then understood the Professor had seen the *lymphatics* in Birds. And Dr. Donald Monro, when I saw him at St. George's Hospital, asked me, if I had not heard that this gentleman (mentioning his name, viz Dr. James Blair, then in London, now in Virginia) had said, that the Professor had then mentioned his having seen the *lacteals* in Birds. I answered Dr. Donald, that I had heard something of the matter, but could conclude nothing from it (or to that effect). The reason was this; I knew from the testimony of my own memory, that the Professor had then acknowledged the contrary of his ever having seen the *lacteals*†. I knew the same from the testimony of gentlemen

* Dr. Morgan's note proves he did so, see above, page 209.

who had attended his lectures since. I therefore concluded that this gentleman had confounded his saying he had seen the *lymphatics*, with his saying he had seen the *lacteals*, which I thought might easily happen, as I never knew him take any notes. And upon receiving the Professor's letter, I wrote to Dr. Blair, and in his answer he acknowledged, "that although he had, indeed, for several years, been under a general persuasion that Dr. Monro had seen the *lacteals* or *lymphatics* in fowls, yet he had no note on the subject, and a very confused remembrance of what he had heard."

SIMILAR to this accusation is the greater part of a letter which I received from Dr. Monro, in answer to two of mine. This letter is dated Sept. 30, 1769. The Doctor has not printed it, but I beg leave to take a little notice of it.

He

He begins it by altering the sense of his assertion read before the Royal Society, by the introducing the word *believed*, making it rather a doubtful than a positive assertion. He has done the same in the beginning of his State of Facts, qualifying the alteration, by adding, "*to the best of his recollection,*" "*and that he had not kept a copy of his letter, not supposing it material to do so*.*" But surely this was not sufficiently qualifying it. If he did not know exactly what he had then asserted, why, before he defended it, did he not ask a copy from his brother, who, most probably, would keep it, in order to *show it to every body*; or solicit that favour from the secretaries of the Royal Society where it was read, who, he might be sure, had preserved it, as they do every paper that is laid before the Society. And again, if he was not sure

* See State of Facts, p. 5.

of its contents, how could he now venture, in his State of Facts, *positively* to insist, in opposition to what I had declared, "that his first and last assertions were exactly the same*." This at least was inexcusable.

NEXT, he repeats his vague inferences as in his letter of the 8th of June, "that he had seen what he *suspected* to be "those vessels, &c." and afterwards, when he comes to speak of the conclusions concerning his claim, which I made in my letter read before the Royal Society, he says, "That he was almost "ashamed, on my account, to add a "plain corollary, that I must or might "have been conscious, that the injurious "conclusion with respect to him, which "I was labouring to impress on the "members of a respectable Society, was "drawn from arguments that were "weak, inconclusive, not real, but

* State of Facts, p. 26, in the note.

"feigned."

“feigned*.” Afterwards he tells me,
 “that he is glad, *on my account as well*
 “*as his own*, that I am at last really
 “ashamed of my letter.” And he then
 finishes with the following passage:
 “Another unhappy mistake of yours
 “(says he) is, that you should not have
 “known, or rather perhaps misfortune
 “of yours, since you don’t seem to have
 “known so much, that you should not
 “have been told, that your presuming
 “to draw the above conclusion concern-
 “ing any person who had the smallest
 “pretence to character, without pro-
 “ducing proof and absolute certainty of
 “its being true, was what you never
 “could be able to justify to any gentle-
 “man.”

Now, when it is considered that Dr.
 Monro obliged me to act in the manner
 I have done, in order to secure my

* The conclusions alluded to in these passages are
 printed above, p. 175, and 176.

right,

right, do not these passages appear very extraordinary? — But the reader, I believe, will excuse my not dwelling upon them. I shall therefore only add, that the *proofs* on which my *conclusions* were founded, being now laid before the public, to their judgment I willingly submit them, and that, with respect to Dr. Monro, I have nothing more to say, than that I hope *for his sake as well as my own*, to see no more of his *claims*, his *assertions*, and his *conclusions*.

THE END.

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